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TABLE OF CONTENTS

ALICE M. BATTY
Some Observations upon Practice and Fatigue as they Affect the Rate of Tapping 449-455

MADISON BENTLEY
Some New Apparatus, I. An Observation Table . 278-279

CHARLES S. BERRY and ROBERT M. YERKES
The Association Reaction Method of Mental Diagnosis (Tatbestandsdiagnostik) 22-37

MARY WHITON CALKINS
The Abandonment of Sensationalism in Psychology . 269-277

ETHEL CHAMBERLAIN and J. H. LEUBA
The Influence of the Duration and of the Rate of Arm Movements upon the Judgment of their Length . 374-385

P. R. DEAN, E. L. THORNDIKE, and W. LAY
The Relation of Accuracy in Sensory Discrimination to General Intelligence 364-369

HAVELOCK ELLIS
Sexual Education and Nakedness 297-317

L. R. GEISSLER and E. B. TITCHENER
A Bibliography of the Scientific Writings of Wilhelm Wundt 570

L. R. GEISSLER
A Critique of Professor Wirth's Methods of Measurement of Attention 120-130

L. R. GEISSLER
The Measurement of Attention 473-529

CEPHAS GUILLET
Retentiveness in Child and Adult 318-352

REVERE RANDOLPH GURLEY
Chapters for a Biological-Empirical Psychology . 60-106

EDMUND B. HUEY
The International Congress of Psychology . . 571-575

F. KUHLMANN
On the Analysis of Auditory Memory Consciousness . 194-218

WILFRED LAY, E. L. THORNDIKE, and P. R. DEAN
The Relation of Accuracy in Sensory Discrimination to General Intelligence 364-369

JAMES H. LEUBA
An Apparatus for the Study of Kinæsthetic Space Perception 370-373

JAMES H. LEUBA
On Three Types of Behavior 107-119

JAMES H. LEUBA and E. CHAMBERLAIN
The Influence of the Duration and of the Rate of Arm Movements upon the Judgment of their Length . 374-385

92544

M. McMEIN and M. F. WASHBURN
The Effect of Mental Type on the Interference of Motor Habits 282-284

ELSIE MURRAY
Organic Sensation 386-446

TAIZO NAKASHIMA
Contributions to the Study of the Affective Processes . 157-193

WILLIAM HENRY PYLE
An Experimental Study of Expectation . . . 530-569

K. B. ROSE
Some Statistics on Synæsthesia 447

EDWARD L. THORNDIKE, WILFRED LAY and P. R. DEAN
The Relation of Accuracy in Sensory Discrimination to General Intelligence 364-369

E. B. TITCHENER
A Demonstrational Color Pyramid 15-21

E. B. TITCHENER
The Psychophysics of Climate 1-14

E. B. TITCHENER
Some New Apparatus. II. A Demonstrational Stereoscope 279

E. B. TITCHENER and L. R. GEISSLER
A Bibliography of the Scientific Writings of Wilhelm Wundt 570

E. B. TITCHENER and G. M. WHIPPLE
Some New Apparatus. III. Tuning-Forks for Tests of Pitch-Discrimination 279-281

ALMA DE VRIES and M. F. WASHBURN
A Study of Retinal Rivalry in the After-Image . . 131-135

CHARLES W. WADDLE
Miracles of Healing 219-268

MARGARET F. WASHBURN
An Instance of the Effect of Verbal Suggestion on Tactual Space Perception 447-448

MARGARET F. WASHBURN and M. McMEIN
The Effect of Mental Type on the Interference of Motor Habits 282-284

MARGARET F. WASHBURN and ALMA DE VRIES
A Study of Retinal Rivalry in the After-Image . . 131-135

FREDERIC LYMAN WELLS
Sex Differences in the Tapping Test: An Interpretation 353-363

FREDERIC LYMAN WELLS
Studies in Retardation as given in the Fatigue Phenomena of the Tapping Test 38-59

G. M. WHIPPLE and E. B. TITCHENER
Some New Apparatus. III. Tuning-Forks for Tests of Pitch-Discrimination 279-281

ROBERT M. YERKES and CHARLES S. BERRY
The Association Reaction Method of Mental Diagnosis (Tatbestandsdiagnostik) 22-37

PSYCHOLOGICAL LITERATURE . 136-154, 285-295, 456-468, 576-592

BOOK NOTES 154-155, 593-596

NOTES 156, 296, 469-472

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THE PSYCHOPHYSICS OF CLIMATE

By E. B. TITCHENER

I owe to Professor Cleveland Abbe a number of references to papers of psychophysical import, published for the most part in meteorological journals, which so far as I am aware have escaped the notice of experimental psychologists. Such further search as I have been able to make has brought to light other papers of like character, and I have no doubt that a systematic student of meteorological 'literature' would discover still more. I desire, however, in the present note, simply to call attention to a few points of importance, and to print a very tentative bibliography.

The 'method of classification' or 'method of the psychophysical series', which in theory goes back to Fechner's essay *Ueber ein psychophysisches Grundgesetz und dessen Beziehung zur Schätzung der Sterngrössen*[1], is usually referred, as a method of laboratory practice, to Ebbinghaus (1887) and Jastrow (1888).[2] I find, however, that as early as 1876 experiments were made by J. W. Osborne, of Washington, D. C., upon the classification of sensible temperatures.[3] Osborne's method "consists in obtaining from a sufficiently large number of intelligent persons, their individual estimate of the sensible temperature for certain fixed hours in each day, and then deducing means from the whole record. . . This was accomplished by conceiving the total range of sensible climatic temperature to be divided into twenty equal parts, counting upwards from the extreme of cold. To each of these divisions

[1] *Abhandlungen d. kgl. sächs. Ges. d. Wiss.*, iv., 1859 [1858], 457-532. See my *Exper. Psychol.*, II., ii., 1905, 419.
[2] See my *Exper. Psychol.*, II., ii., 1905, 85, 89, 91.
[3] Determinations of Subjective Temperature, in *Proceedings A. A. A. S.*, 25 Meeting (August, 1876), 1877, 66 ff.

a descriptive expression was affixed conveying in as unambiguous a way as possible, the idea of a progressive elevation of temperature at every step. The observer, in the open air, and sheltered only from the direct rays of the sun, selects the expression which most nearly describes his appreciation of the sensible temperature at the time, and records, not the expression, but the number attached to it. From such records, means and generalisations can be obtained which increase in value as the observers increase in numbers, and gain experience''. The scale of observations, as printed on the outside of the record-card, is as follows: 20 Intolerably hot; 19 Excessively hot; 18 Very hot; 17 Tolerably hot; 16 Very warm; 15 Decidedly warm; 14 Agreeably warm; 13 Mild and soft; then, after an interval, 12 Mild and fresh; 11 Quite fresh; 10 Very fresh; 9 Decidedly cool; 8 Very Cool; 7 Moderately cold; 6 Cold and fine; 5 Cold and sharp; 4 Very cold; 3 Bitterly cold; 2 Painfully cold; 1 Unbearably cold. A foot-note adds: ''However qualified, these expressions must be understood to refer to equal gradations of sensible temperature only''. Under the heading of 'Remarks' are given three scales of letters, which provide for a general characterisation of the weather in terms of wind, humidity and sunshine.

The results obtained from 25 to 35 observers over a period of 9 weeks from June to August are shown in tables of daily observations and means and of weekly means. The writer points out that, ''as was to be expected, the observations do not always coincide for the same day and period [time of day]. This is due to the fact that the system at best is but approximate, that individuals differ in their estimates of temperature, and that living in different localities, some exposed to one wind, and some to another, they are not affected similarly. Nevertheless the means obtained from this large mass of somewhat crude material, have exhibited so remarkable a degree of consistency and harmony as to justify great confidence in the method, which certainly furnishes information relative to climate that has not been hitherto obtained''. Unfortunately, no attempt is made in this paper to compare the subjective estimates of temperature with the ordinary meteorological records. What is significant, however, from the psychophysical point of view, is the writer's assumption (roughly verified by the facts) that the average educated man or woman is able to 'place' a given experience of climatic temperature upon a subjective temperature-scale made up of 19 equal sense-distances between the limits 'intolerably hot' and 'unbearably cold'.

Another and perhaps a still more striking instance of 'psychophysics by common sense' is found in two papers by W. F. Tyler, entitled respectively ''A Scheme for the Comparison of

Climates"[1] and "The Psycho-Physical Aspect of Climate with a Theory Concerning Intensities of Sensation".[2] The writer's aim is to lay down a scale of some sort for the intercomparison of climates, as regards, *e. g.*, their bracing or relaxing character. "It has been assumed that the principal factors forming climate are some five in number", rainfall, soil, altitude, aspect, wind. These five may be reduced to the four: humidity, temperature, sunshine, pressure. And these, again, may practically be reduced to two: humidity and temperature. "It seems likely that temperature and humidity are so incomparably more important and of more effect than the others, that, eliminating these other comparatively unimportant factors, a law may be found (sufficient for practical purposes) connecting the sensation scale with temperature and humidity alone".

"The observations were made as follows: A number of persons of normal condition and regular habits were requested to estimate daily at noon the degree of 'hyther' on a scale of 0 to 10. The word hyther was introduced to indicate the sensation caused by a warm climate [the original observations were made at Shanghai], and supposed to be due to the combined effect of heat and humidity. *Ten* represents the very worst day an observer remembers to have experienced in Shanghai—hot, damp and enervating; while 0 represents an ideal summer's day—warm of course, but bright, brisk and bracing, when, suitably dressed, one suffers no discomfort from temperature and humidity". The hyther or 'hydrotherm' is thus taken as the unit of mugginess.

As to the possibility of a sensation scale, "it is maintained that the mind has an innate, but generally dormant, faculty of subdividing a sensation or emotion". "Many, if not most sensations, have instrumentally measurable conditions corresponding to them in their varying intensities, and the whole object of the writer's idea in regard to sensation scales generally is the establishment of the laws connecting equal differences of these sensations with the instrumental variations corresponding to them, and thereby enable (*sic*) the formation of scales in terms of instrumentally measurable quantities, the degrees of which correspond to equal differences of sensation". A terminology is then worked out. The innate faculty of the mind to graduate is termed indicativeness. "*Sensation* is the physiological effect, of which we are cognizant, of one or more physical causes. A *Sensation Scale* is a means whereby the

[1]Journal of Balneology and Climatology, VIII, January, 1904, 17 ff.
[2]London, John Bale, Sons and Danielsson, Ltd. 1907. Reprinted from the *Journal of Tropical Medicine and Hygiene*, April 15, 1907. This second paper in large measure repeats the earlier essay of 1904.

intensity of a sensation can be named. A *Sensation Increment* is the minimum appreciable alteration in the intensity of a sensation. A *Physical Increment* is the amount of variation of the physical cause producing a sensation increment". Sensation increments are equal (1904, p. 23; 1907, p. 9) and may be summed up; the physical increment is "a variable quantity, and by analogy alone it might be assumed that it is some function of the intensity of the physical cause". "*Equal Differences of Sensation* are those which have the subjective effect of a similar degree of change".[1]

Here surely is Fechner *redivivus*,—although Mr. Tyler has never heard of Fechner! The adoption of the just noticeable difference as the unit of sensory measurement; the doctrine that all just noticeable differences of sensation are equal; the contention that just noticeable differences (or other unit-differences of sensation) may be summed up; the standardisation of the sense-scale by reference to a corresponding stimulus-scale: all these things are commonplaces to the reader of the *Elemente*. The writer even vacillates, as Fechner does, between the notion that the single sensation is a measurable magnitude and the notion that it is merely the limiting term of a sense-distance; though it is fair to say that he seems to come nearer than his unknown master to the modern point of view. And indeed, if it were worth while, still other and more detailed parallels might be drawn between Mr. Tyler's positions and the traditional teaching of the psychophysicists.[2]

Fechner, as we all know, at first believed himself to be the discoverer of the law which is called by Weber's name, and only later found that he and Weber had, both alike, been anticipated. And he lays especial emphasis on the century-old classification of the fixed stars by visible magnitude. "Es

[1] All of these definitions, except that of equal differences of sensation, are taken from the paper of 1904. In 1907 the sensation scale is defined as "a progression of stimulus intensities such that the differences of corresponding sensation between any consecutive pairs are equal to one another" (p. 9).

[2] The psychophysics of the paper of 1904 was worked out "quite independently," without any sort of technical knowledge of what the author, in his blindness, still calls "the little known subject of intensity of sensation" (1907, 1). Before printing his second paper, Mr. Tyler had been made aware of Weber and Fechner, and had read Landois and Stirling's "Text Book of Human Physiology", Schäfer's "Text Book of Physiology", Mercier's "Psychology, Normal and Morbid", and Ladd's "Psychology, Descriptive and Explanatory". All, alas! secondary sources. He has, however, "seen no reason to alter anything he has written in consequence of what he has read", though he has "embodied in the paper [of 1907] extracts from the authorities named, with his own remarks concerning them",—remarks that are both instructive and amusing.

giebt . . . eine . . . Bewährung des Gesetzes an mehr als nur eben merklichen Unterschieden, zugleich die erste, die überhaupt für das Gesetz existirt, und zwar wiederum auf jenem hohen Beobachtungsfelde, dem die zuerst angeführten Bewährungen entnommen wurden, nämlich in der Schätzungsweise der Sterngrössen".[1] Mr. Tyler, now, finds outside confirmation of his 'sensation scales', of precisely the same kind. He appeals, first, to Beaufort's scale of wind force. This is a scale of twelve numbers introduced into the British Navy about the year 1805 by Admiral Sir F. Beaufort (1774-1857), for use in recording the apparent strength of the wind. The 0-point of the scale denotes calm, the 12-point a hurricane. "The general impression, even among meteorologists of eminence, appears to be that, because Captain Beaufort distinguished between wind forces according to the amount of sail his vessel [a full-rigged man-of-war] could carry, therefore the division of his scale was a mere arbitrary one. The writer's idea, however, is that, probably unconsciously, he selected twelve wind forces which differed from one another by an 'equal difference of sensation' and he in effect used the sails carried merely as identifying labels". Eventually, the velocities corresponding to Beaufort's numbers were ascertained, as the means of a large number of estimates; and Mr. Tyler shows that "these velocities are, with only one exception, within 1.5 miles, functions of the corresponding Beaufort numbers, being expressible by the formula:

$$v = 3 + 5n + \frac{n(n-1)(n-2)}{60}$$

where v is the velocity and n a Beaufort number. This indicates with certainty that the estimate of the wind force by Beaufort's numbers was not according to some arbitrary method, but that there is a definite relationship between the several intensities of sensation. For want of a better way of expressing it the writer says that sensations due to the different degrees of Beaufort's scale differ from one another by an equal degree of sensation". How Beaufort actually obtained his scale appears not to be certainly known; "several professional meteorologists" have accepted Mr. Tyler's interpretation (1907, p. 4).

A second bit of confirmatory evidence—which is, however, of a much more doubtful character, and upon which Mr. Tyler accordingly places less reliance—appears in the graduation of bath thermometers. "Certain bath thermometers are marked according to Dr. Forbes' Specification. Concerning who Dr. Forbes was and how he arrived at his graduation the writer

[1] *Elemente der Psychophysik*, i., 1889, 158.

has no knowledge.[1] It always appeared to him that these graduations were obviously empirical. A comparison, however, of them with those obtained [in the writer's own experiments with cold and warm water] by the method of physical increments shows a partial conformity in regard to differences which perhaps points to more than a mere coincidence''. Mr. Tyler, in fact, finds equal sense distances represented by 27, 10, 6 and 4 degrees F., where Forbes' Specification has Cold-Cool, 21 degrees; Cool-Temperate, 12; Temperate to Tepid, 9; and Warm to Hot, 7 degrees.[2]

It is time, however, that we returned to our hythers. With them it fared about as well as could have been expected. The several hyther numbers are plotted on diagrams, in which the temperatures are laid off along the axis of x and the differences between the wet and dry bulbs along the axis of y. In Mr. Tyler's own case, the numbers group themselves into fairly regular zones; in the case of four other observers the zones are distinctly indicated; in the case of seven observers we can say no more than that the higher and lower numbers are divided by a line, and that in all instances the line has approximately the same slope. If, however, the data from the eleven assistants are combined in a single diagram, the resulting curve is very similar to Mr. Tyler's; "the remarkable conformity between these two curves tends to show that the estimates of No. 1 observer [the writer] were taken with considerable accuracy. There is rarely as great a difference as 1 [one hyther number] between them.'' "As far as any conclusion can be come to on

[1] The Dr. Forbes in question is no less a person than Sir John Forbes, F. R. S. (1757-1861), whom I am proud to claim, in virtue of his long residence in my native city, as a fellow Cicestrian. See *The Cyclopædia of Practical Medicine*, edited by J. Forbes, A. Tweedie and J. Conolly, i., 1833, art. Bathing, p. 245. Forbes merely says that his scale is "founded on practical indications". It is laid out, not in single values (as on current bath thermometers), but in ranges; cold 33-60, cool 60-75, temperate 75-85, tepid 85-92, warm 92-98, and hot 98-112 degrees F.

[2] At the risk of too much detail, I cannot refrain from mentioning that Mr. Tyler attempted to construct a subjective temperature scale by two different methods: that "of estimating relative intensities in respect to two standard intensities",—the method of the psychophysical series, or the method of equal sense distances; and that of deducing "the relative intensities by means of experimentally ascertained 'least observable differences'",—the method which has been modernised as the method of limits. "And now", he says, "is brought to light a disappointing fact. The two methods of forming a sensation scale result in two different scales". Not only, then, did Mr. Tyler invent two of the recognised metric methods of psychophysics, but in seeking to correlate their results he has found, in strictly orthodox fashion, the discrepancy that we connect chiefly with the name of Merkel. A step further, and we should have had him discussing absolute impression, and the *R*-error, and Merkel's Law!

the very limited data provided, it would appear that temperature and humidity are certainly the factors of paramount importance in our appreciation of climate, but that some other factor or factors occasionally have appreciable effect." Mr. Tyler was unable to connect observations for pressure, wind or nebulosity with the irregularities in the hyther numbers. He offers the tentative formula:

$$H=\frac{d-1.2\ (d-w)-66}{3},$$

where H indicates hyther on a scale of 0 to 10 (indicates, that is, the degree of discomfort due to the mugginess of the day), and d and w represent respectively the readings of the dry and wet bulb thermometer. Since for comparatively high humidities d and w are approximately the same, the formula may be simplified to:

$$H=\frac{w-66}{3},$$

and the degrees of discomfort are thus brought into direct relation to the wet-bulb readings.

It would be superfluous to print, in this *Journal*, a technical criticism of Mr. Tyler's views, methods and results. To the psychologist, the significant thing with him, as with Mr. Osborne, is the effort of psychophysical construction. But it is not superfluous, I hope, to urge the better acquaintance of the psychophysicist and the meteorologist. Experimental psychology has already begun, from its own point of view, the study of the weather: Dexter's *Conduct and the Weather* came out in 1899, and Lehmann and Pedersen's *Das Wetter und unsere Arbeit: experimentelle Untersuchungen über den Einfluss der meteorologischen Faktoren auf die körperliche und seelische Arbeitsfähigkeit* came out in 1907. Now we learn that the meteorologists have these many years been engaged, for their purposes and from their point of view, upon the problem of 'subjective climate' or 'sensible temperature'. The solution of that problem, however, demands a combination of the two techniques and of the two standpoints. I have no doubt that solution, at any rate in the rough, is possible with the means at hand. Something can be done by the mutual printing of essays and reviews in the professional journals, by the exchange of papers, by personal correspondence. But I should like to see some of the younger generation of climatologists pass through the drill of the psychological laboratory, and I should like to see some student of experimental psychology take up the meteorological problem. The running analysis of the titles given below in the bibliography may help to indicate precisely where that problem lies.—

Just as I am about to send the MS. of this note to the printer, I receive the *Zeits. f. Psychol.*, xlix., Heft 3-4, which contains an article by J. Plassmann on "Astronomie und Psychologie." It is clear that the technical 'literature' of astronomy offers to the psychophysicist a mine of unworked materials at least as valuable as those that I have found in the works upon meteorology. The extraction and refining of the metal, in both cases, will require time and labor; but it is greatly to be hoped that some interested student may undertake the task.

References[1]

1826. W. Heberden, An Account of the Heat of July, 1825; together with Some Remarks upon Sensible Cold. *Phil. Trans. Roy. Soc. Lond.*, 1826, pt. II, 69-74. [Suggests that the way to estimate 'sensible cold' would be to warm a thermometer (on the assumption that the radiating surface of the instrument is analogous to that of the body) "to a height something exceeding the natural heat of the human body, and then to observe at what rate the quicksilver contracted upon exposure to the air".]

1853. J. C. Houzeau, *Règles de climatologie*, pp. 59 f. [Proposes to take the readings of the wet-bulb thermometer as measures of sensible temperature.]

1860. C. Martins, Des causes du froid sur les hautes montagnes. *Annales de chimie et de physique*, 3d series, lviii., pp. 208-243. [Emphasises the importance, for subjective temperature, of the wind and of the direct action of the sun, in addition to the temperature of the air.—See also Du froid thermomètrique et de ses relations avec le froid physiologique. *Montpellier Acad. Sci. Mém.*, IV., 1858-60, pp. 251-301. Conditions subjectives qui modifient la sensation du froid. *Brown-Sequard's Journ. de Physiol.*, III., 1860, 597-603.]

1873. J. C. Houzeau, *Patria Belgica*, 1re partie, p. 12. [Again recommends the wet-bulb thermometer, and gives tables.]

1876. J. W. Osborne, On a New Meteorological Instrument. *Proceedings A. A. A. S.*, 24 Meeting (August, 1875), 1876, Part 1, Physics, 59-70. [Refers sensible climatic heat to the three factors: temperature of the air, relative humidity, and force or velocity of wind. Proposes to measure sensible temperature by the time required for a mass of water, heated to the temperature of the human body, and contained in a paper cylinder of special construction, to cool under varying conditions.]

1877. J. W. Osborne, Determinations of Subjective Temperature. *Proceedings A. A. A. S.*, 25 Meeting (August, 1876), 1877, 66-74. [The paper referred to in the text. The author, at the end, expresses the hope that he may be able, on a future occasion, to compare his subjective results with the readings of the instrument described in the previous year. I have not been able to find any such comparison.—See also *Bulletin of the Philos. Soc. of Washington*, ii., p. 63; *Smithsonian Miscellaneous Collections*, xx., 1881.]

[1]For aid in the compilation of this bibliography I am especially indebted to the Messrs. Cleveland Abbe, Sr. and Jr., to the librarian of the Surgeon-General's Office, Washington, D. C., and to Professor R. de C. Ward, of Harvard University.

1879. G. Forbes, On Observations relative to the Temperatures to which the Human Body is Exposed. *Journal of the Scottish Meteorological Society*, N. S., V., 1880, Nos. 49-62, pp. 273-4. [Describes an instrument similar to that of Osborne, and devised for the same purpose.]

1882. A. Borius, *Les maladies du Sénégal*, Baillière, Paris. [I have not seen this work. According to Vincent, 1907, the author argues (pp. 124-240) that the depressing or relaxing character of tropical climates is due to excessive humidity. He proposes, but does not use, a sensation scale: 0 Very cold, 5 Indifferent, 10 Extremely hot. Intermediate degrees are not defined.]

1885. C. Abbe, Meteorology. In *Smithsonian Report* for 1883, 483-569. [Report of scientific progress during 1883, with bibliography. Declares that 'exhilarating' and 'depressing' weather may be regarded as functions of air temperature, wind, barometric pressure and relative humidity. Refers to Osborne: p. 491; advocates special observation of days in which the human organism experiences such special feelings as are defined by the expressions: close, oppressive, harsh, raw, penetrating, chill, mild, soft, soothing, invigorating, exhilarating, stimulating, balmy, gloomy, cheerful, nervous, restless.]

1887. C. Smart, The Thermometer as a Climatological Instrument. *Trans. Internat. Medical Congress, 9th Session*, Vol. V., pp. 172-178. [Determines the combinations of air-temperature and wind-velocity that produce, in the unit of time, the same drop of the thermometer (from 98.4° F.) as a given degree of cold in a calm atmosphere. Argues that the "cooling effect" or the "demand on the powers of the system" in the two cases is the same.]

1890. J. Vincent, La détermination de la température climatologique. Brussels, 1890. Reprinted from *Annuaire de l'Observatoire Royal pour 1890*. Abstract in *Ciel et Terre*, 10e année, 1889-1890, p. 515. [I have not seen this paper. According to Hann, pp. 43 f., "Vincent undertook an extended series of investigations in order to show the relation between the temperature of the exposed surface of the skin (T); the air temperature (t); the excess of the temperature as indicated by the actinometer over that of the air, in degrees (d); and the velocity of the wind in meters per second (v)." He found the approximate formula:

$$T = 26.5° + 0.3° t + 0.2° d - 1.2 v.$$

The degrees are C. According to Van Bebber, p. 133, Vincent established further a seven-degree scale of temperature sensation as follows: 1 Very hot, copious sweat, discomfort, T over 37.5°; 2 Hot, sweat, little or no discomfort, T 34.5°-37.5°; 3 Warm, no sweat, T 31.5°-34.5°; 4 Moderate, indifferent condition,—one may sit in the open air without an overcoat,—T 29°-31.5°; 5 Fresh, cold, but not unpleasantly cold to the hands,—one cannot sit in the open without a coat,—T 26°-29°; 6 Cold, unpleasant to the hands, T 22°-26°; 7 Very cold, unbearable to the hands and unpleasant to the face, T under 22°.]

1889. H. F. Blanford, *Climates and Weather of India, Ceylon and Burmah*, Macmillan & Co., London, p. 48. [Contrasts the oppressiveness of a humid Red Sea temperature of 90° with the dry hot-wind season of the upper provinces of India at a temperature of 112° to 118°, when the conditions, "if not exactly agreeable, are borne without serious inconvenience."]

1892. M. W. Harrington, Climate and Meteorology of Death Valley, California, *U. S. Dept. of Agriculture, Weather Bureau Bulletin, No. 1*, pp. 50. [Remarks, p. 23, that the "most interesting

form of humidity by which to judge of the comfort to be found in residence at a place" is the relative humidity. The low relative humidity of the valley, together with the large air-motions, causes evaporation to proceed with very great rapidity. Again, p. 30, "a good measure of the rigor of a climate is to be found in the mean daily range of temperature." The diurnal ranges are great in the valley, but are equalled and surpassed elsewhere.]

1892-3. A. Piche, Le déperditomètre. *Assoc. française pour l'avancement des sciences*, Session de 1892, i., 195; ii., 296-300. [Describes an instrument that should measure the volume of gas or alcohol consumed in maintaining for a definite length of time a known volume of water at a uniform temperature of 37° C. under varying meteorological conditions.—The description is repeated in the *Bulletin mensuel de l' Observatoire Carlier d' Orthez*, 7e année, No. 1, January, 1901.]

1894. M. W. Harrington, Sensible Temperatures. *Intern. Med. Mag.*, iii., August, 481-485. Also published separately, as paper read before the American Climatological Association, Washington, D. C., May, 1894: pp. 7, with Discussion and three Plates. Abstract in *Amer. Metl. Journ.*, July, 1895, 93-95. [Sensible temperatures depend on evaporation; and when evaporation takes place they are invariably lower than the shade temperatures given in meteorological tables. In actual practice, the temperature of evaporation is taken by means of a wet-bulb thermometer. The author gives illustrative charts of (1) the reduction of meau temperatures over the United States, for a given period, due to evaporation, and (2) the resulting sensible temperatures.]

1894. C. Abbe, Humidity. *Monthly Weather Review*, xxii., 407 f., 453 f., 496 f. [Note on wet-bulb or sensible temperatures, with references. Printed for the first time in Oct., 1894; reprinted, with slight modification and abridgment, till Octr. 1895, when doubt is cast upon the correlation. In xxiii., 368, we are told that "a complete ezpression for the relation between atmospheric conditions and nervous sensations is under consideration, but has not yet been obtained." See also Summary for 1895, xxiii., 491; the wet-bulb readings are given for "the sake of certain studies in Hygiene." In xxiv., Jan. 1896, 3 the statement is made that the sensation of temperature depends on the temperature of the air, its dryness, the velocity of the wind, and the suddenness of atmospheric changes, all combined with the physiological condition of the observer. A complete expression of sensible temperature has not yet been obtained. See also Summary for 1896, xxiv., 488.]

1895. W. J. van Bebber, *Hygienische Meteorologie*. Enke, Stuttgart, pp. 124-145. [Discussion of the "Hygienische Bedeutnng der Wärmeerscheinungen." Very hot temperatures are endurable, if the air is dry and, more especially, if it is in motion; a moist day is oppressive with a temperature of only 30° C. (p. 130). Dry-air baths do no harm at 80°, while steam-baths may be injurious at 50° or less (p. 136). The injurious effect of low temperatures is due to the cold, to the dryness of the air (ct. Hann, pp. 54 f.), and to a violent air-motion (p. 138); yet we are especially liable to take cold in damp, tropical climates and in a climate "in welchem *nass*kaltes und windiges Wetter vorherrscht" (p. 143). Heat-prostration is due mainly to air-temperature and humidity; windlessness also plays a part (p. 137).]

1895. C. Abbe, Sensible Temperatures. *Monthly Weather Review*, xxiii., March, 93 f. [Historical note.]

1895. B. S. Pague, Sensible Temperatures, or the Effect of Heat on the Body in California. *Amer. Metl. Journ.*, xii., No. 6, Oct., 1895, 196-198. [Equates wet-bulb readings with sensible temperatures. Applies this correlation to the climate of Death Valley, etc. Sunstroke is due to heat and humidity.]

1896. W. F. R. Phillips, Sunstroke weather of August, 1906. *Monthly Weather Review*, xxiv., Novr., 409-413. [Besides heat, "evidently there must enter into the case another factor, namely, the accommodation of the individual to average physical environment, or the climatic equation."]

1896. W. F. R. Phillips, Sunstroke in California and Arizona. *Monthly Weather Review*, xxiv., Dec., 454-456. [These sunstrokes are "not caused by the traditional high relative humidity." References.]

1896. W. F. R. Phillips, Sensible Temperature. *Trans. Amer. Climatol. Assn.*, xii., 16-25. [Opens with an historical review of the subject. The writer correlated Osborne's subjective results with the synchronous meteorological observations, but "was unable to discover more than a very general agreement." He admits, nevertheless, that the uniformity of the records "does appear to suggest something more than accident." He discusses "the principal factors concerned in the determination of temperature sensation" and shows incidentally "why failure (to establish an instrumental register of sensible temperature) has so far been inevitable."]

1896. W. L. Moore. Some Climatic Features of the Arid Regions. *U. S. Dept. of Agriculture, Weather Bureau*, pp. 19, with 5 Plates. Communication to National Irrigation Congress, 5th annual session, Phœnix, Ariz., Decr. 15-17, 1896. ["The meteorological instrument that registers the temperature of evaporation and thus in a great measure the actual heat felt by the human body, is the wet-bulb thermometer." On Sept. 20, 1895, instructions were issued "to the observers of the weather service to begin the telegraphing from observation stations of the readings of the wet-bulb thermometer, more popularly known as the 'sensible' temperature. This is about the temperature felt by animal life." Charts are appended, showing the distribution of average actual and sensible temperatures over the United States.]

1896. W A. Glassford, Why Summer in the Apparently Hot Arid Region is Comfortable. *Monthly Report, Oregon State Weather Service*, Jan., 1896, 23-29. [Equates 'sensible temperature' with the reading of the wet-bulb thermometer.]

1896. I. M. Cline, Influences of Climatic Conditions and Weather Changes on the Functions of the Skin. *Proc. Texas State Med. Assn.* Reprint, pp. 8, with a Plate. [Considers the functions of the skin, as influenced by weather changes and climatic conditions, under the headings of Secretion and Excretion, and Regulation of the Evolution of Heat. Both sets of functions are affected mainly by temperature and absolute humidity. The influence of these climatic factors is discussed in detail for the six types of climate in the United States: low damp warm, low damp cold, high dry, between low damp warm and low damp cold, between low damp warm and high dry, and between low damp cold and high dry.]

1897. W. F. R. Phillips, Clothing and Temperature. *Monthly Weather*

Review, xxv., May, 200-201. [Thermometrical observations, paralleled by estimate of subjective sensations. References to Rubner and van Bebber.]

1897. M. Rubner und V. A. Lewaschew, *Archiv f. Hygiene*, xxix., pp. 1 ff. Abstract in *Meteorologische Zeitschrift*, xv., 1898, pp. 148 f. [I have seen only the abstract of this paper. It contains a report of experiments on the human subject, carried out in the respiration chamber. At low temperatures, 14-15° C., dry air is more comfortable than moist. Between 24° and 29° dry air appears cooler than moist; high temperatures are not uncomfortable if the air is very dry; visible sweat begins at 29° with a relative humidity of 22%. A relative humidity of 96% makes a temperature of 24° unendurable for any length of time. Evaporation (loss of water-vapor) is a continuous function of temperature both in dry and in moist air, but the course of the function varies very considerably with the relative humidity.]

1898. C. Abbe, Sensible Temperatures and the Curve of Comfort. *Monthly Weather Review*, xxvi., August, 362 f. [Suggests that diagrams be prepared, with a vertical scale of relative humidities from 0 to 100 and a horizontal scale of air-temperature from *minus* 10 to 100, and that the observer make a record whenever he feels like saying: "Well, this weather is just perfect."]

1899. C. Abbe, Sensible Temperatures. *Monthly Weather Review* xxvii., 18. [Brief comment on a suggestion in the *N. Y. Times* that the Weather Bureau "combine the figures indicating temperature, humidity and velocity of the winds into a single figure that would express just what people mean when they say and feel that the weather is hot or cold."]

1899. A. Lancaster, *De la manière d'utiliser les observations hygrométriques*. Rapport lu au Ve Congrès International d' Hydrologie, de Climatologie et de Géologie Médicales à Liège, 1898. Vaillant-Carmanne, Liège, 1899. [A comparative study of the combined effect of air-temperature and relative humidity at Brussels and at Vivi in the Congo Free State. The author sought to determine, in calm weather at Brussels, what combinations gave certain high temperatures, in his own case, an oppressive feeling of heat. He gives a numerical table of results. According to Tyler, 1907, 31, "Lancaster's oppression curve coincides very closely with the position which the curve for Hyther 1 would occupy. Now Hyther 1 is a condition when very lightly dressed in a hot climate one begins to feel discomfort. The same condition in Belgium experienced by an observer with heavy European clothing might well be said to be oppressive."]

1900. E. G. Ravenstein, The Geographical Distribution of Relative Humidity. In *Report of 70th Meeting of Brit. Ass. Adv. Sci.*, 817-818. [In temperate weather we can bear great humidity with equanimity, while the same degree of humidity accompanied by great heat may be unbearable. The writer maps the earth according to sixteen hygrothermal types of climate.]

1903. J. Hann, *Handbook of Climatology. Pt. i., General Climatology*. Translated by R. de C. Ward. New York, The Macmillan Co., 1903. pp. 43-46, 82 f. [Brief review of previous work on sensible or subjective temperature. The temperature which we actually experience depends upon air temperature, air movement, insolation and the humidity of the air. "In hot climates, and also in the summer of middle and higher latitudes, when the body is usually covered with perspiration, the temperature which is actually felt depends to a great extent upon the dry-

ness of the air, or, to put it more plainly, upon the reading of the wet-bulb thermometer. . . In the case of hot climates it would be advisable to include, among the climatic elements, the readings of the wet-bulb thermometer as a convenient index of the degree of heat which is actually felt by the human body". Refers critically to Abbe's proposal of 1883 (to classify days as harsh, raw, mild, etc.) and to Osborne's sensation scale of 1876. States that both very hot (p. 45) and very cold (pp. 54 f.) climates are more endurable when the air is dry.]

1904. W. F. Tyler, The Sensation of Discomfort. *Monthly Weather Review*, xxxii., 217. [Letter written from Shanghai, communicated by the editor, who adds comments on the writer's Scheme for the Comparison of Climates, and refers to Osborne. The editor remarks that the use of the wet-bulb reading as a measure of sensible temperature has "long since been given up."]

1904. W. F. Tyler, A Scheme for the Comparison of Climates. *The Journal of Balneology and Climatology*, viii., January, 1904, pp. 17-44, with plates. Not offprinted: price of number 2/-. [Paper referred to in the text.]

1904. R. de C. Ward, "Sensible Temperatures." *Bulletin of the American Geographical Society*, xxxvi., no. 3, 129-138. [Summary of factors concerned in sensible temperature. General review of work done, with appended bibliography.]

1906. R. Börnstein, *Leitfaden der Wetterkunde*, 2d ed., Braunschweig, p. 48. [Emphasises the importance of relative humidity for sensible temperature.]

1907. W. F. Tyler, Hythers and the Comparison of Climates. *Monthly Weather Review*, xxxv, June, 267 f. [Letter written from Shanghai in 1905, briefly discussing the 'comfort curve', the use of hythers, etc. The editor appends a bibliography.]

1907. W. F. Tyler, The Psycho-Physical Aspect of Climate with a Theory concerning Intensities of Sensation. London, John Bale, Sons & Danielsson, Ltd. Pp. 45, with plates. 5/-. Reprinted from the *Journal of Tropical Medicine and Hygiene*, April 15, 1907. [Paper referred to in the text.]

1907. J. Vincent, Nouvelles recherches sur la température climatologique. *Annales météorologiques* de l'Observatoire royal de Belgique, année 1907, nouvelle série. In extract, pp. 120. Abstracts in *Ciel et Terre*, xxviii., 1907, p. 25; *Annuaire météorologique* de l'Observatoire royal, 1908, p. 462. [In this paper Vincent modifies his formula of 1890 to read (with the omission of the *d*-values, which are discarded as empirical only):

$$T = 30.1 + 0.2t - v\,(4.12 - 0.13t).$$

This formula "permet de calculer la température de la peau à l'ombre, lorsque l'on connait la température de l'air et la vitesse du vent. On ne doit y recourir que lorsque t est supérieur à 17°".—The most important point of the paper, however, is the ruling out of humidity as a factor of sensible temperature, under the conditions of the author's observations. His general conclusion reads: "lorsque la température superficielle de la main de l'homme est inférieure à celle qui provoque la production de sueur visible, ce qui est le cas le plus fréquent dans les climats tempérés, l' humidité de l'air n'a aucune influence sur notre sensation thermique et ne doit pas être considérée dans l'étude de cette sensation." And he writes in detail: "l' humidité de l'air . . . . n'a aucune influence sur la température superficielle de notre corps, dans les conditions atmosphériques

ou nous avons opéré. . . . Le thermomètre mouillé, même immobile, suit bien les fluctuations de l'humidité; il devrait en être de même de la surface de notre corps, si cette surface était réellement comparable à un linge mouillé; mais qui ne voit qu'en général elle ne l'est pas? . . . . La capacité de l'air ambiant pour la vapeur d'eau, capacité que la chaleur propre du corps élève et empêche de devenir nulle, est toujours suffisante, quelle que soit l'humidité relative, pour vaporiser immédiatement et complètement l'eau qui atteint la surface de notre corps. Lorsque la température de la peau vient à s'élever suffisamment sous l'action des influences atmosphériques, les glandes sudoripares deviennent très actives et déversent à la surface de la peau la sueur qu'elles ont secretée abondamment. Alors seulement notre corps peut être comparé jusqu'à un certain point au thermomètre mouillé, puisqu'il s'y accomplit une évaporation dans le sens habituel du mot, sur l'activité de laquelle l'humidité de l'air doit, cette fois, exercer une influence. Il ne faudra, en tout cas, jamais perdre de vue que notre corps a sa chaleur propre, ce qui constitue une différence importante avec le thermomètre mouillé. . . . D'ordinaire, notre peau ne peut être assimilée à un linge mouillé: cela est clair, puisqu'elle est presque parfaitement sèche. On raisonne pourtant à son propos comme si elle était toujours couverte de sueur. . . . La température de la peau mouillée est notablement inférieure à celle de la peau sèche; elle est, par contre, supérieure, et de beaucoup, à celle du thermomètre mouillé; les différences sont, du reste, loin d'être constantes." Vincent further discusses, with approval, Heberden's indirect method, and gives a bibliography of previous work, with critical notes. He advises that, whatever form of objective method be adopted, the observer should append to his records a statement of the subjective temperature in the terms 'very hot', 'hot', 'warm', 'mild', 'cool', 'cold' and 'very cold.' This scale may be roughly standardised as follows: Very hot, 37.5° and over; Hot, 34 5° to 37.4°; Warm, 32.4° to 34.4°; Mild, 27° to 32.3°; Cool, 26.9° and under. The point of transition from Cool to Mild is uncertain; the points of transition from Cool to Cold and from Cold to Very cold are still more uncertain. These determinations have, however, little practical importance, as we meet cold weather by appropriate clothing.]

A DEMONSTRATIONAL COLOR-PYRAMID

By E. B. TITCHENER

It is usual, in text-books and lecture courses, to represent the system of visual qualities by means of some tridimensional figure, sphere or double cone or hexahedron (double three-sided pyramid) or octahedron (double four-sided pyramid). I have long felt the need of a model of this figure,—a large model that should show, by actual pigment, the distribution of hue and tint and chroma in the visual system. In the course of a series of lectures on light and color, delivered during the summer of 1908 to a class of art-students, I was fortunate enough to secure the interest and cooperation of Mr. Louis Agassiz Fuertes, whose skill as a colorist requires no advertisement. For the immediate needs of the class, we constructed in cardboard a small water-colored model of the regular octahedral double pyramid; and this model did such good service that we judged it worth while to build a larger and more durable pyramid for regular use in class-room and laboratory.

The earliest colored *chart* with which I am acquainted is Lambert's single three-sided pyramid (J. H. Lambert, Beschreibung einer mit dem Calauschen Wachse ausgemalten Farbenpyramide, etc., Berlin, 1772). Aspects of the color-sphere are shown in colors by Runge and Steffens, in their Farbenkugel oder Construction der Verhältnisse aller Mischungen der Farben in der Natur, Hamburg, 1810. Many books contain colored plates that may, at a pinch, be useful to the lecturer: I mention, *e. g.*, the chromatic scale of tones printed as Plate v., and the chromatic circle of hues printed as Plate vi., in Chevreul's Principles of Harmony and Contrast of Colors, 3d ed., London, 1890. The Prang charts and Nendel's Farbenkreis should also be referred to; and the painted metal plate of equated Urfarben and grey, contained in Hegg's Die invariablen aequivalenten Oelpigmente zur Farbenperimetrie, is indispensable. Since, however, the system of visual qualities is in fact tridimensional, colored charts can never be wholly satisfactory; they give, at the best, a series of typical aspects which must be imaginatively combined by the spectator.

In this connection I am tempted to remark that the two-dimensional representation of a tridimensional system in Ross' Theory of Pure Design (1907) has, in my opinion, brought the author into serious conflict with psychological fact. Ross admits three variables in the color impression, value, color and intensity (tint, hue and chroma). His classificatory diagram 4 (facing p. 140) is, however, essentially two-dimensional, though it can be translated into a color pyramid if we ignore the extreme length of the lines R-Y and B-R (due apparently to the existence of the color-names orange and violet) and the

extreme lowering of V (due in part, perhaps, to the exigencies of the diagram itself, and in part to the darkness of V in the ordinary dispersion spectrum: see p. 139). No one can quarrel in principle with empirical rules of procedure; but one may object to the distortion of theory, *i. e.*, in this case, of psychological observation.[1] There seems, indeed, to be not even an empirical reason for the lowering of V; such, at any rate, was Mr. Fuertes' experience in the painting of our model.

A *model* of the double cone, 12 in. from pole to pole and 8 in. in diameter at the base, was put upon the market ($4.00) by E. G. Willyoung & Co. in 1896. I am informed that the model showed no color, but simply copied, in black and white, Fig. 88 of Scripture's Thinking, Feeling, Doing, 1895, p. 158 (*cf.*, however, The New Psychology, 1897, pp. 345 ff., Figg. 87, 88). Two models, a "Farben-Oktaeder, zerlegbar in die acht Oktanten" and a "Farben-Doppeltetraeder", are listed by Höfler and Witasek in their Psychologische Schulversuche mit Angabe der Apparate, Leipzig, 1900, p. 4 and 1903, p. 5. The former "zeigt am Umfange des horizontalen Achsenschnittes die gesättigten Farbentöne (Grund- und Mischfarben), an den Spitzen der vertikalen Achsen Weiss und Schwarz, an den Innenschnitten die Anteile verschiedener Grau an den nicht gesättigten Farben". Details of construction are promised, in both editions, for a forthcoming number of the *Zeitschrift f. Psychol.*, but the article has not appeared. The cuts suggest small models of painted wood or plaster; the price (Kr. 10.60 and Kr. 4 respectively: say $2.25 and $0.85) seems to indicate that the colors and greys are only roughly represented.

The octahedral double pyramid, which embodies the fundamental facts of the 'antagonistic' theory of visual sensation, was figured in 1897 by Höfler and Ebbinghaus. Höfler's pyramid has the form of a regular octahedron (*Psychologie*, 1897, p. 113, Fig. 12; *Grundlehren der Psychologie*, 1897, p. 35, Fig. 9). Ebbinghaus brings the figure into better accordance with psychological requirement by tilting the Y-angle of the base up towards W, and by rounding off the poles and base-angles (*Grundzüge d. Psychol.*, erster Halbband, 1897, p. 184, Fig. 15; or i., 1905, p. 199, Fig. 18). We decided to tilt the base of our model, partly because the relative displacement of Y and its antagonistic B helps the student to grasp the meaning of 'tint' as applied to color, and partly because the painting of the model in this form is much easier; theory and practice are, so far, in agreement. We did not, however, round off the poles and the corners of the base. There would have been no appreciable gain, that we could see, on the technical side; the model would have been more expensive; and there would have been incorporated in it a point of exposition which, as experience shows, may more safely be postponed. Our own pyramid, then, as painted by Mr. Fuertes, finally

[1] *Cf.* A. H. Munsell: A Color Notation, 1905, 12. "Two dimensions fail to describe a color. Much of the popular misunderstanding of color is caused by ignorance of these three dimensions or by an attempt to make two dimensions do the work of three." Munsell uses hue, value and chroma for the three qualitative attributes that I term hue, tint and chroma.

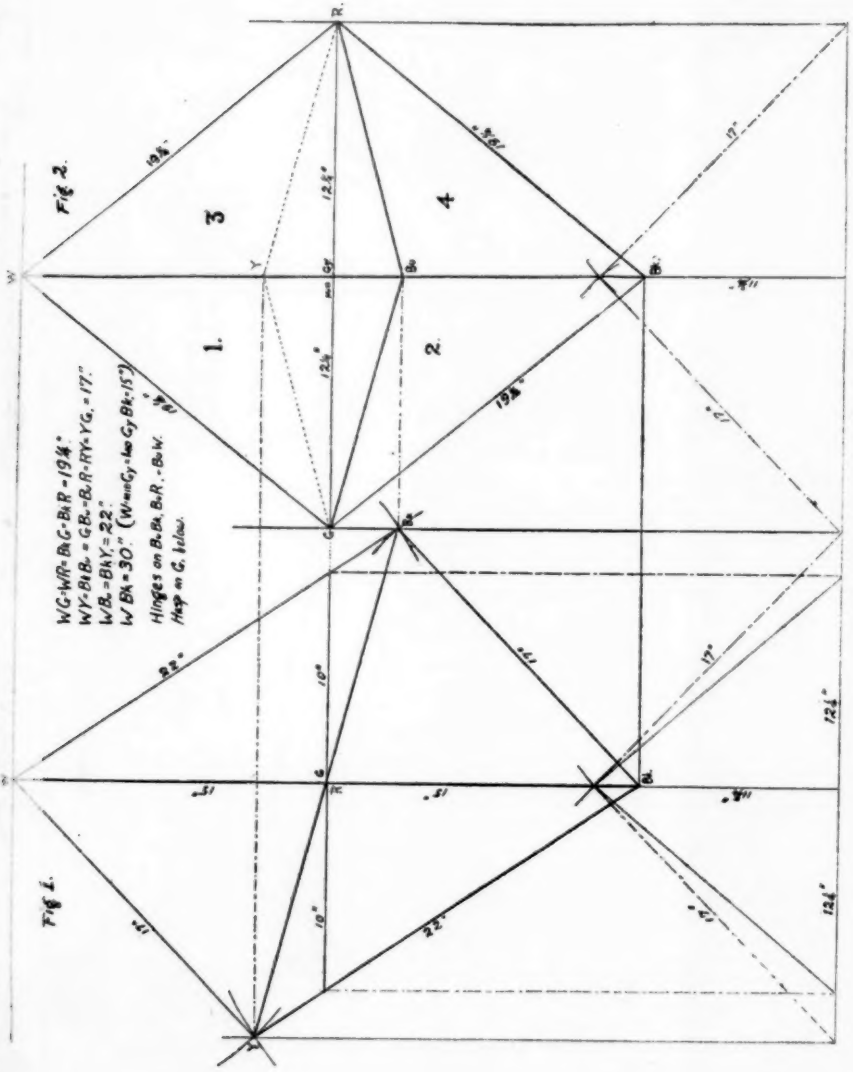


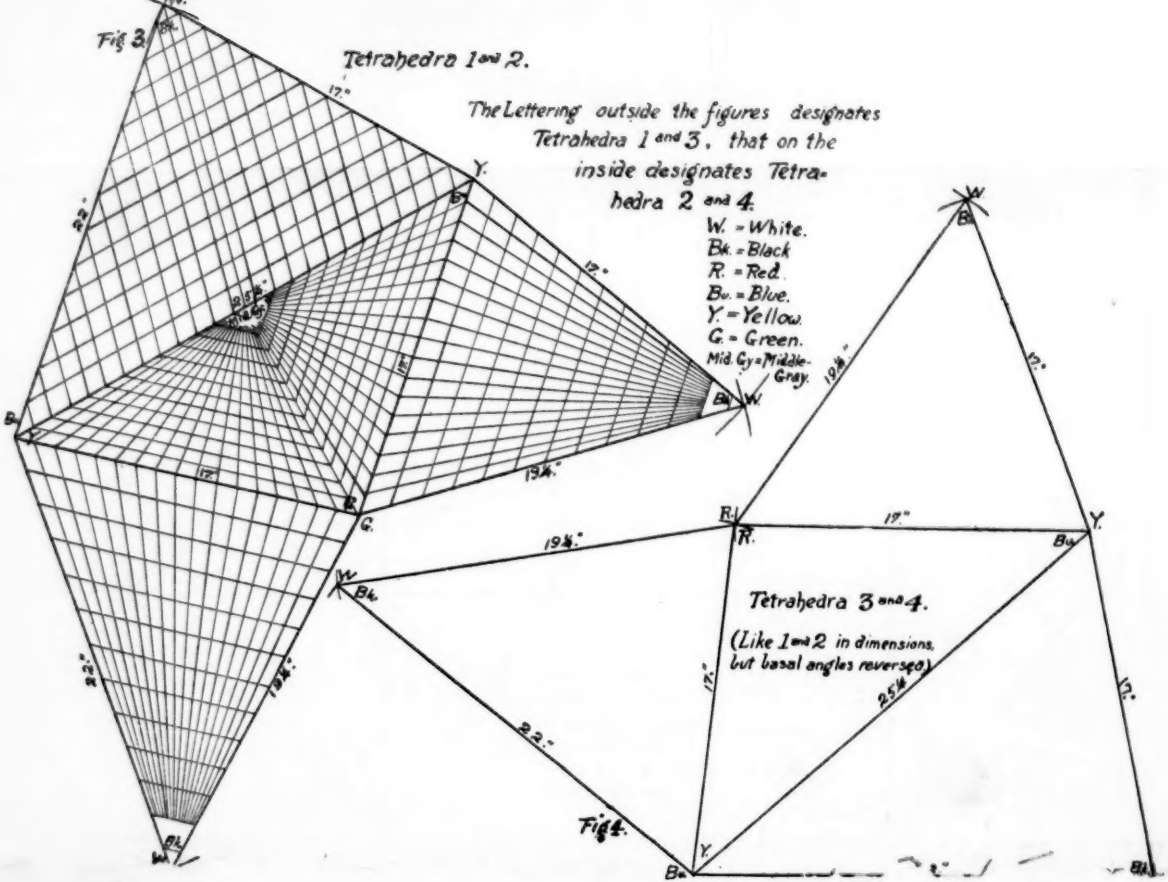
Fig 2.
Fig 1.
WG=WR=BrG=BrR=19½"
WY=BrBl=GBl=BlR=RY=YG=17"
WBl=BrY=22"
WBr=30° (W=Gy=Lt Gy Br=15°)
Hinges on Br Bl, Br R, Br W.
Hasp on G, below.
1.
2.
3
4
W
Y
G
R
Br
Bl
19½"
17"
22"
12½"
10"

Fig 3.

Tetrahedra 1 and 2.

The Lettering outside the figures designates Tetrahedra 1 and 3, that on the inside designates Tetrahedra 2 and 4.

W. = White.
Bk. = Black
R. = Red.
Bu. = Blue.
Y. = Yellow.
G. = Green.
Mid. Gy = Middle-Gray.



Tetrahedra 3 and 4.

(Like 1 and 2 in dimensions, but basal angles reversed)

Fig 4.

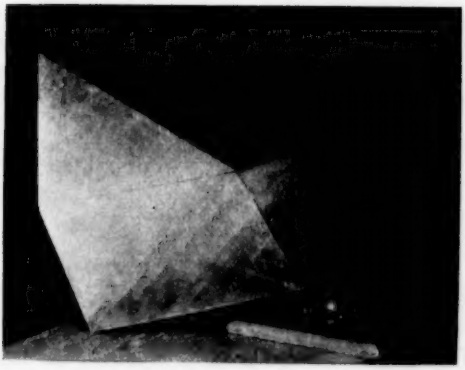


PLATE I

assumed the form shown in the accompanying photographs. I append details of its manufacture, in the hope that they may be of value to other psychologists in lecturing to large undergraduate classes: the model saves at least a full hour of classroom time, and a good deal of unnecessary puzzling on the part of the student. In case the cost still appears excessive, I suggest that the figure may easily be constructed (from the plans here given) of heavy white cardboard, with white cloth hinges, and that a very moderate degree of skill in water-coloring will suffice to produce a serviceable model.

To construct the unit-tetrahedra, lay off the triangle BuYG: BuY = 25.25, YG = 17, GBu = 17 in. On BuY as a base, lay off BuW = 22 and YW = 17 in. On YG as a base, lay off YW = 17 and GW = 19.25 in. Finally, on GBu as a base, lay off GW = 19.25 and WBu = 22 in. These four triangles placed together will make tetrahedra 1 and 2; with the same measurements, but with the angles reversed, they will make tetrahedra 3 and 4. The four tetrahedra, fitted together as in Figg. 1 and 2, form the double pyramid required.

The material used is specially selected, trebly seasoned 'state' whitewood, ⅝-in. thick. Attached firmly to the angle GBk is a forked walnut standard which, seated at the base in a strong swivel socket, allows the model to be revolved about a vertical axis. The standard is fixed to a cast-iron base, 16 by 16 by 1.5 in. Two small pin-hinges, set as closely as possible to the surfaces, are placed on the edge BuBk; two others on BuR; and two others on BuW. These, while holding the model together in one piece, allow of its being opened to show a vertical section through W, Bu, Y, Bk and to show a peripheral section through R, Y, G, Bu and middle grey. A single pin-hasp, hooking from G slightly below the periphery and across the edge BkG, locks the entire model.

To ensure uniformity of gradation in the coloring, the entire surface of all the tetrahedra was plotted, as shown in Fig. 3, on a unit of one inch, taken on the sides of the base triangles and thence projected upon the remaining three faces. On the surfaces of section Bk, Bu, W, Y, the figures thus formed are rhomboids which decrease by twos in the direction from base to apex; on all the other surfaces the figures are rhomboids which show the same number (17) in all rows. The plotting was done with a hard pencil-point on the raw wood.

To prepare for the coloring, the whole of the upper pyramid and the upper half of the lower pyramid were first painted with a good coat of flake white mixed in pure boiled linseed oil. The lower part of the lower pyramid was simply given a thorough oiling, the white being brushed out evenly along the line of junction. After this treatment, the model was allowed to stand for three weeks. It was then entirely dry, and no color could 'sink' after application.

The colors used were J. H. Hatfield's hand-ground oil colors, and the palette was selected by Mr. Hatfield with special reference to our requirement of pigments that should produce purest hues of maximal chroma, and should not interact chemically or change on long exposure to light. The paints selected were: zinc white, flake white, ivory black, vert emeraude, aureolin yellow, light cadmium yellow, cadmium orange, orange vermilion, fast deep vermilion, rose madder, cobalt violet, ultra-marine blue, American cobalt blue and French cobalt blue. These colors, lightened with white only, make it possible to render the outer surfaces at high chroma, even at very light tints; they also furnish a high chroma for the lower pyramid. It was

found that the blues required a good deal of lifting even to reach the 'pure blue' level.—The addition to the above list of a tube of burnt sienna simplifies the complicated mixing for dark yellows, which tend strongly to fall over towards green or red.

The first step of the painting was to work out carefully the colorless series along the BkW axis. This was laid off in 25 stages, with pure W and pure Bk at the poles, and 23 graded neutrals between. Enough of the neutrals was mixed to modify all the chromas of Bu and Y, between the limits of black and white, on the vertical section, and enough of middle grey to modify all the hues of the peripheral section.

In painting the outside faces, the periphery was first established, all four tetrahedra being thus far carried on together. The start was made at Y with light cadmium, which was taken to express the maximal chroma of yellow. This was worked, step by step, into cadmium orange, pure, and thence through orange vermilion into fast deep vermilion, which expresses a nearly perfect red at maximal chroma. The f. d. vermilion was then worked through rose madder (with white) and cobalt violet to cobalt blue (with white) which forms the note at Bu. This mixture gives a very good middle blue, which works evenly into a green-blue with the addition of vert emeraude, which in turn is nearly perfect (needing only a little white) for blue-green. If the white is now gradually replaced by light cadmium, an excellent high-chroma green is obtained for G; and serial reduction of the green and increase of the light cadmium gives the GY line. These colors form the first row on both planes of all edges of the periphery.

The faces towards W are produced simply by serial addition of white to the notes already established. Those towards Bk offer a much more serious problem; the mechanical mixture of black with the hues of the periphery does not result in the required gradations. Red must be worked down through rose madder *plus* orange vermilion, with black added where necessary. Blue, as the white component of Bu is diminished, becomes darker in tint without loss of chroma to a point nearly (if not quite) half-way towards Bk. Vert emeraude is nearly as dark, and must be treated in nearly the same way. The green at G can be developed down a few steps by reduction of the light cadmium; then this must be replaced by aureolin *plus* cadmium orange; and then the black increment must begin. The series downward from G-Y is the most difficult and complicated of all, and must in large measure be left to the skill and eye of the painter; the changes, as the different colors are employed, are very instable, and the proportions must be sought empirically. Especial care must be taken with the colors between green-yellow and Y, if they are to be kept clear, and not to lean over towards red or green. A little burnt sienna will tend, as was said above, to warm and clear the difficult series of ambers and browns between Y and Bk; it also works well into the dark oranges between YBk and RBk.

The painting of the sections (both surfaces of which were covered) was comparatively easy. On the vertical section, the chroma of the peripheral blues and yellows was decreased by adding the serial increment of the neutral found at the middle line in the same row. On the peripheral section, very little besides middle grey was required to neutralize all the hues; when the grey component becomes predominant, the color lies so near the horizontal plane that middle grey may be used without appreciable error. To have laid other sections through the model would have impaired its stability, and would have added considerably to its cost. On the other hand, there is no difficulty, when once the faces described have been painted, in coloring

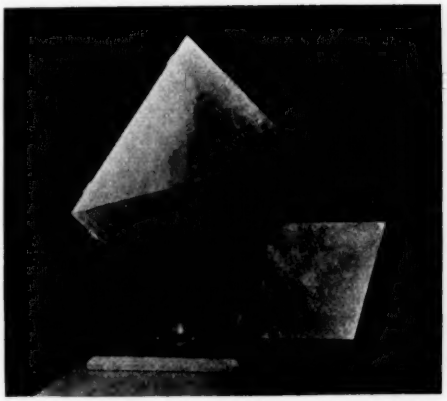


PLATE II

other surfaces that represent planes of section in any direction. We have worked out only one of these: the horizontal plane that passes through R, G, and middle grey, and therefore also through a high blue and a low yellow. A separate rhombus of wood was made to the required dimensions; the hues were laid off along the periphery by reference to the painted pyramid; and the surface, showing all hues in all chromas at the tint of middle grey, was treated in the same way as those of the peripheral section. It is probably unnecessary to prepare more of these sectional surfaces; students who have followed the construction of the pyramid on the blackboard, and have been led to the complete model by way of the Hegg plate and the separate rhombus, find no special difficulty in following imaginary lines and covering imaginary planes within the color system.

The price of the wooden model was $26, and of the iron base, $3; the cost of the pigments, brushes, etc., a little over $7. Given the necessary skill in painting, the model can therefore be reproduced for $37.

Explanation of Plates

Plate I. Color-pyramid, closed, seen from back. Shows pin-hasp at G, and forked support.

Color pyramid, open. Shows vertical section.

Plate II. Color pyramid, open. Shows peripheral section.

Color pyramid, closed, seen from front, the R-angle facing observer. Separate rhombus (horizontal section) at side.

THE ASSOCIATION REACTION METHOD OF MENTAL DIAGNOSIS (*Tatbestandsdiagnostik*)

By ROBERT M. YERKES and CHARLES S. BERRY

In connection with a course in Applied Psychology, given in the Harvard Summer School during the session of 1908, the association reaction method of mental diagnosis was employed for the purpose of class demonstration. The discovery of this method of studying the content of consciousness is claimed by both Wertheimer and Jung,[1] who, with other psychiatrists and criminologists, have discussed its applications and values from several points of view. Without previous experience in the use of the method and with little hope of success in our efforts to adapt it to the demands made upon a class demonstration experiment, we planned and executed the experiments whose results constitute the material of this report. The success which rewarded our efforts to demonstrate the value of the method to a class of elementary students of Psychology would seem to justify a brief account of our work. We wish to have it clearly understood, however, that we are not attempting a critique of the method of Jung and Wertheimer, and that we do not consider our results in themselves as of special importance. It is our aim merely to call attention to a useful adaptation of the method in the teaching of Psychology.

EXPERIMENT I

We attempted, in our first experiment, to discover which of two possible acts a person had performed. In a room of the laboratory we placed two small boxes, one of which contained a dancing mouse, and the other a pack of playing cards. The boxes were covered, so that their contents could be discovered only by raising the lids. In another room of the laboratory, the classroom, we had arranged a Hipp chronoscope, electrically connected, with a single hand key in the circuit. After the class had assembled in this room we explained briefly what we proposed to do, selected one member of the class to serve as subject for the experiment, and then directed that individual, in the company of the remainder of the class, to go to the room which contained the boxes, open one of them, examine its contents, and, without disturbing the other box, return to

[1] For the literature on "Tatbestandsdiagnostik" see bibliography at the end of this article.

the classroom to be tested by the association reaction-time method.

We had previously prepared two lists of thirty words each. In one of them occurred several words which referred directly to the contents of the mouse-box. This we may hereafter call the mouse-box list. In the other occurred certain words which similarly referred to the contents of the card-box. This we may therefore designate as the card-box list. These lists, of experiment 1, appear in tables 1 and 2.

The subject, upon returning to the classroom, was given a seat at one side of the table which carried the chronoscope. On the opposite side sat the experimenter, whose task it was to call the words of the list and manipulate the reaction key. At the end of the table, so seated that he could readily read the chronoscope, was an assistant experimenter. The subject having been directed to conceal, if possible, knowledge of the contents of the box which had been opened, was told to respond to each word of the list, as it was pronounced by the experimenter, by speaking as quickly as posssble the idea which came into consciousness.

The experiment was then conducted as follows. The chronoscope having been set in motion by the assistant, the experimenter gave a ready signal and then pronounced distinctly the first word of the mouse-box list, at the same instant closing the key of the chronoscope circuit and thus starting a reaction-time record. The instant the subject pronounced an associated word (reaction word) the experimenter released the key and thus stopped the recording hands of the chronoscope.[1] Immediately the assistant read and recorded the position of the hands on the dial of the chronoscope. As soon as this had been accomplished, the experimenter pronounced the second word of the mouse-box list, and so on until the chronoscope had run down. Usually five reaction times could be measured without

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[1] The experimenter was an experienced reactor and the uniformity of his reactions made it seem desirable that he react to both the stimulus word and the reaction word. Had the subject instead of the experimenter reacted to the reaction word the reaction times might be somewhat shorter, but they would also be less reliable because of lack of previous training.

It may be objected that the experimenter's knowledge of the contents of the boxes influenced his reaction times, and thus determined the results of the experiment. We appreciated and met this danger in our second experiment, for in it the experimenter had no knowledge of what the subject had done and was unable to distinguish the significant from the irrelevant words of the list. As our results were positive in both experiment 1 and experiment 2, we feel confident that the experimenter's knowledge of the conditions of the first experiment did not render the results unreliable.

interruption for winding the instrument.[1] As soon as the mouse-box list had been completed, the card-box list was given in the same way.

Upon the completion of this initial experiment we tabulated and examined our results in the presence of the class, and, in the light of them, we were able to state with considerable confidence that the subject had opened the mouse-box instead of the card-box. As this conclusion proved to be correct it will be worth while for us now to examine the facts upon which our judgment was based.

In the accompanying lists of words some, it will be noticed, are marked with an asterisk. They refer to the contents of the box. To distinguish them from the other words of the series, which are properly described by the term *irrelevant*, we may speak of them as *significant*. The mouse-box series contained eight significant and twenty-two irrelevant words, but as the word squeak was misunderstood by the subject there were in effect only seven words which referred to the contents of this box. We planned to have one significant word to two irrelevant words in each series.

Beside the number of each word given (stimulus word) and the word itself the tables present the associated words (reaction words) given by the subject, and the reaction time. The latter we may, in order to be on safe ground, consider as reliable only to the second decimal place. Careful study of the associations which the two lists of words called out gives us little if any suggestion as to which box had been examined. But when we turn to the reaction times we meet with a strikingly different condition. As is shown in the general results for experiment 1, the average or mean for the irrelevant words of the mouse-box list was 1.775″ while that for the significant words was 2.487″. Apparently the significance of the words rendered their reaction times considerably longer than they would ordinarily have been. For the card-box list the difference between the reaction time for the two groups of words is only 0.125″. Furthermore it is important to note that the variability, as well as the range, of reaction time is greater for the mouse-box list than for the card-box list.

Turning to individual words and their reaction times, we

[1] It has occurred to us since these experiments were performed that the Münsterberg chronoscope (readable to hundredths of a second) would be more satisfactory than the Hipp, because it can be used for ten minutes or more without interruption for winding. Professor Münsterberg, who, in his employment of the association reaction method for diagnostic purposes has made use of a stop-watch which could be read to fifths of a second, states that his readings are sufficiently accurate for practical purposes.

note that the irrelevant word rain, number 19 of the mouse-box list, was reacted to with the word clouds in 2.419″, that the five consecutive significant words, small, white, dance, tail and rat, were reacted to with the words faucet, sink, book, horse and cat in 2.981″, 2.830″, 2.233″, 1.948″, and 3.208″ respectively, and finally, that the immediately succeeding irrelevant word book was reacted to with the word paper in only 2.205″. The word rat, following as it did a series of significant words, caused an inhibition or delay of reaction which was very noticeable indeed.

The attitude of the subject toward the experiment is indicated by the following brief introspective report. "I tried to give, as I understood I was expected to, the first association that came to mind. I endeavored, however, at the same time,

TABLE I

Mouse-Box List of Experiment 1

No. of Word	Stimulus Word	Reaction Word	Reaction Time
1	House	Dog	1.670 sec.
2	Sky	Blue	1.067
3	Tree	Green	1.340
4	Sun	Yellow	1.740
5	Flower	Blue	1.293
6	Boston	City	1.637
7	Man	Tall	.955
8	Harvard	Yard	1.758
9	Knife	Cut	1.600
10	Psychology	Study	1.675
11	*Box	Wood	2.117
12	*Movement	Machine	2.090
13	Squeak[1]	Sugar	3.146
14	Woman	White	1.906
15	Drink	Water	1.733
16	Room	Wood	2.098
17	Study	Book	1.381
18	Consciousness	Brain	2.234
19	Rain	Clouds	2.419
20	*Small	Faucet	2.981
21	*White	Sink	2.830
22	*Dance	Book	2.233
23	*Tail	Horse	1.948
24	*Rat	Cat	3.208
25	Book	Paper	2.205
26	Money	Color	1.206
27	Crime	Murder	2.205
28	Ground	Yellow	2.206
29	Companion	Woman	1.667
30	Home	House	1.697

[1] This word was understood to be sweet; hence the associated word sugar.

TABLE 2

Card-Box List of Experiment 1

No. of Word	Stimulus Word	Reaction Word	Reaction Time
1	Iron	Metal	1.731 sec.
2	Wolf	Animal	1.666
3	Wine	Red	1.296
4	Salad	Lettuce	1.845
5	Railroad	Rails	2.379
6	Orange	Yellow	1.214
7	Ticket	Railroad-train	2.612
8	Journey	Steamer	1.499
9	Lonesome	Child	3.109
10	*Play	Earth	2.344
11	*Ace	Cards	1.480
12	*Diamonds	Brilliant	1.894
13	*Clubs	Ace of Clubs	2.690
14	Trump[1]	Box	3.116
15	Wind	Blow	2.057
16	Window	Light	1.416
17	Dog	Yellow	2.100
18	Apple	Red	1.475
19	Fish	Swim	1.706
20	*Box	Trunk	2.146
21	*Lid	Trunk	2.271
22	*Open	Book	1.384
23	Black	Coat	1.072
24	*King	Queen	2.025
25	*Jack	Cards	1.398
26	*Hearts	Cards	1.798
27	Sleep	Tired	1.664
28	Morning	Light	1.387
29	Home	House	1.365
30	Friends	Many	1.655

to put out of mind all thought of my experiences with the boxes."

In the light of the reaction times of this experiment there seemed to be good reason for concluding that the subject had opened the mouse box: hence our statement to that effect.

The unexpectedly definite and convincing results of this preliminary experiment led us further to test the value of the method in a slightly different way three days later, with the same class. In the meantime the members of the class, being deeply interested in the experiments, had discussed with the assistant experimenter and among themselves the method, our results, and various ways in which a subject might render the method useless. It is significant of the importance of the

[1] This word was understood to be trunk; hence the associated word box.

TABLE 3

Quantitative Results of Experiment 1

	Reaction-Time Values for Mouse-Box List		Reaction-Time Values for Card-Box List		
	Irrelevant Words	Significant Words	Irrelevant Words	Significant Words	
Mean	1.775″	2.487″	1.818″	1.943″	
Mean Var.	.366	.445	.449	.352	
Difference in Means	+0.712		+0.125		
Extremes Short	.955	1.948	1.072	1.384	
Extremes Long	2.914[1]	3.208	3.109[2]	2.690	
Max. Range	2.253″		1.618″		

method, that, despite this discussion, it continued to yield positive results.

EXPERIMENT 2

Our task in experiment 2 was to discover which of two subjects had performed a certain series of acts. With the apparatus arranged as previously, and with the list of one hundred words which appears in table 4 prepared, we chose two members of the class, Mr. L. and Mr. W., as subjects. These individuals were then directed to take a folded note and leave the classroom. When out of sight and hearing of those of us who remained in the classroom they were to decide which one of them should open the note and carry out certain written directions which it contained. They were clearly told that only one of them should know anything about the contents of the note, and that they should make it their business to prevent the experimenters from discovering which of them had the information that would be gained by following the written directions which are appended.

"Go to the shop, place a hammer in a drawer which is indicated by the mechanic, and carefully examine the articles in the drawer so that you can answer correctly the following

[1]Omitting the reaction time for word number 13 (squeak) because it was misunderstood.

[2]Omitting the reaction time for word number 14 (trump) because it was misunderstood.

questions: How many keys are on the key ring? What is the label on the ink bottle? Is the ink bottle empty? Is the stop-watch running? What is the number on the back of it? How many blades has the knife? Are they sharp? What is the color of the handle? Can you pull small nails with the hammer?"

One at a time, the two subjects returned to the classroom and were tested for their knowledge of this series of questions.

The results which appear in table 4 demand little comment, but they deserve careful study. We may briefly call attention to the chief grounds of our conclusion that Mr. L. instead of Mr. W. carried out the directions.

First, the significant words caused considerable delay of reaction in Mr. L.'s case and none whatever in Mr. W.'s. Second, the variability of the reaction time to the significant words was greater than that for the irrelevant words in Mr. L.'s case, whereas it was less in Mr. W.'s case. Third, the range of reaction times was considerably greater for Mr. L. than for Mr. W. And, fourth, certain significant words caused very obvious inhibition in the reactions of Mr. L.

Especially important in this experiment are the reactions of Mr. L. to the three significant words Carter's, stopper, blotter which immediately followed the irrelevant word hawk (No. 21). For the latter the reaction time was only 1.873″, for Carter's it increased to 2.279″, for stopper it was 2.059″, for blotter 3.386″, and for the irrelevant word meat, which followed blotter, it suddenly dropped to 2.129″. The inhibition of reaction caused by the word blotter is explained by the introspective report of Mr. L. He had prepared himself for the experiment by thinking over words which might appropriately be associated with the objects which he had seen. For the word blotter he had, as it chanced, no word ready. It took him completely by surprise, and his reaction time was so long that he at once decided that his case was lost. This change in Mr. L.'s attitude toward the experiment manifested itself in the reaction times to the remaining words of the list. The results of the second part of table 5 show that after the first twenty-five words, among which occurred the words stopper, Carter's, and blotter, the significant words of the list caused little delay in reaction. These averages for the reaction times of the series by twenty-fives are worthy of careful examination. Among the first twenty-five words of the list there were twelve significant and thirteen irrelevant words. For the former the reaction time was more than half a second longer than for the latter in the case of Mr. L., and a twentieth of a second shorter in the case of Mr. W. The second group of twenty-five words contained eleven significant and fourteen irrelevant words. Mr.

TABLE 4

Results of Experiment 2

	RESULTS FOR MR. W.			RESULTS FOR MR. L.	
No. of Word	Stimulus Word	Reaction Word	Reaction Time	Reaction Word	Reaction Time
1	Canary	Bird	1.021	Dog	1.289
2	Tea	Yellow	.979	Coffee	1.287
3	Bread	White	1.886	Butter	1.528
4	Dog	Animal	1.022	Cat	.876
5	Lily	Green	1.143	Pond	1.353
6	*Bottle	Green	1.227	Water	1.637
7	*Empty	Crack	1.412	Full	1.448
8	*Black	White	1.054	White	2.057
9	*Write	Figure	1.286	Dog	2.024
10	*Fluid	Paper	1.334	Water	1.580
11	Sparrow	Bird	1.229	Bird	1.170
12	Coffee	Black	1.506	Tea	1.655
13	Cow	Milk	1.274	Milk	1.169
14	Plum	Fruit	1.820	Purple	1.733
15	Door	Open	1.243	Closed	1.812
16	*Lock	Open	1.355	Key	2.017
17	*Rusty	Green	1.233	Nails	1.922
18	*Ring	Key	1.439	Key	.824
19	*Number	White	1.105	Two	2.771
20	Window	Open	1.506	Glass	1.543
21	Hawk	Black	1.187	Bird	1.873
22	*Carter's	Ground	1.375	White	2.279
23	*Stopper	Chair	.963	Bottle	2.059
24	*Blotter	Red	1.269	Paper	3.386
25	Meat	Cotton	1.136	News	2.129
26	Horse	Brown	.999	White	1.806
27	*Ticks	Clock	1.203	Watch	1.595
28	Floor	Open	1.562	Wax	1.893
29	Peach	Stone	1.196	Stones	1.795
30	*Keyhole	Open	1.309	Door	1.733
31	Apple	Seeds	1.573	Seeds	1.439
32	Violet	Violet	1.621	Purple	1.649
33	*Face	Brown	1.007	White	1.654
34	*Hands	Door	1.196	Clean	2.225
35	*Stop	Watch	1.120	Clock	2.165
36	*Set	Open	1.213	Gone	1.798
37	Chocolate	Brown	.971	Cream	1.719
38	Potatoes	White	1.496	White	1.463
39	*Blades	Green	1.696	Sharp	1.643
40	Cat	Grass	1.102	Dogs	1.559
41	Hen	Knife	1.589	Scratch	1.589
42	Soda	White	1.194	Water	1.279
43	Sugar	Glass	1.135	Sweet	1.120
44	*Second	Sugar	1.341	Hand	1.811
45	*Hour	Minute	1.331	Minute	1.637
46	Mouse	Cat	1.058	Trap	1.239
47	Orange	Tooth	1.245	Yellow	1.459
48	Ceiling	White	1.180	White	1.509
49	*Sharp	Dull	1.502	Corn	1.592
50	*Color	Color	1.513	Green	1.810

TABLE 4.—(Continued)

Results of Experiment 2

	RESULTS FOR MR. W.			RESULTS FOR MR. L.	
No. of Word	Stimulus Word	Reaction Word	Reaction Time	Reaction Word	Reaction Time
51	Ostrich	Book	1.030	Plumes	1.464
52	Lemonade	Lemonade	1.648	Sweet	1.834
53	Pickles	Ball Game	1.561	Sharp	1.417
54	Elephant	Animal	1.278	Trunk	1.285
55	Daisy	Flower	1.174	Ox-eye	2.168
56	*Open	Closed	1.118	Shut	1.260
57	*Handle	Door	1.359	Blades	2.155
58	*Cut	Rat	1.102	Dog	1.340
59	*Three	Green	1.587	Six	1.900
60	Roof	Wall	2.613	Green	1.828
61	Pike	Toll	1.749	Road	1.595
62	Dove	Round	1.064	Fly	1.619
63	Beer	Floor	1.128	Foam	1.615
64	Soup	Tomato	1.761	Turtle	1.976
65	Wolf	Wall	1.063	Snarl	1.817
66	Grapes	Glass	1.313	Purple	1.705
67	Wind	Hard	1.562	Blow	1.624
68	*Time	Short	1.404	Minute	1.731
69	*Clean	Board	1.355	White	1.420
70	Chimney	Short	1.530	Smoke	1.302
71	Shark	Smoke	1.306	Bite	1.568
72	Wine	Green	1.426	Milk	2.026
73	Chicken	Hen	1.309	Hen	1.463
74	Rat	Cat	1.081	Trap	1.219
75	Cherries	Stones	1.665	Red	1.708
76	*Hatchet	Axe	1.514	Cut	1.368
77	*Drawer	Red	1.270	Open	1.239
78	Whale	Yellow	1.199	Bone	1.638
79	Ale	Wine	1.103	Beer	1.705
80	Pepper	Potatoes	1.314	Sharp	1.817
81	*Pickup	Key	1.160	Drop	1.809
82	*Open	Paper	1.098	Close	1.237
83	Book	Door	1.053	Shut	1.734
84	Place	Table	1.004	Room	1.155
85	Close	Paper	1.056	White	1.260
86	Steps	Pen	1.192	Down	1.346
87	Swan	Pencil	1.236	White	1.279
88	Goat	Table	.929	Butt	1.751
89	Salad	Bird	1.100	Dressing	1.128
90	Water	Tool	1.150	Drink	1.417
91	Figs	Sharp	1.150	Sweet	1.729
92	*Nail	Box	1.185	Door	1.278
93	*Drive	Hatchet	.955	In	1.283
94	*Pull	Crayon	1.013	Out	1.106
95	Ocean	Hinges	1.246	Green	1.386
96	*One	Pencil	1.046	Six	1.585
97	Ship	Pencil	1.004	Sail	1.464
98	*Ten	Hammer	1.144	Dozen	1.336
99	Boy	Door	.940	Girl	1.214
100	Home	Door	1.359	Down	1.765

TABLE 5

Quantitative Results of Experiment 2

	Results for Mr. W.		Results for Mr. L.	
	Irrelevant Words	Significant Words	Irrelevant Words	Significant Words
Mean	1.293″	1.258″	1.551″	1.729″
Mean Var.	.224	.145	.221	.343
Difference in Means,	−0.035″		+0.178″	
Extremes Short	.929	.955	.876	.824
Extremes Long	2.613	1.696	2.168	3.386
Max. Range	1.684″		2.562″	

Means, and Differences in Means, for the series of Reaction Times by quarters.

1st Quarter (25 words)	1.304″	1.254	1.494″	2.000″
Difference	−0.050		+0.506	
2nd Quarter	1.280	1.312	1.537	1.788
Difference	+0.032		+0.251	
3rd Quarter	1.435	1.321	1.644	1.634
Difference	−0.114		−0.010	
4th Quarter	1.127	1.154	1.493	1.360
Difference	+0.027		−0.133	

L.'s reaction time to the former was about a quarter of a second longer than his reaction time to the latter; Mr. W.'s times differed by only thirty-two thousandths of a second. In the third and the fourth groups of twenty-five, the significant words were reacted to by Mr. L. even more quickly than the

irrelevant words. This marked difference in the results for the first and the second halves of the list of stimulus words is intelligible in the light of Mr. L.'s introspective statement. "I tried," he writes, "to put myself into another state of mind in order not to reveal my knowledge of the facts. I thought over words that might be associated with the objects I had seen, and tried to make my association reaction time slow and uniform. I had not thought of the blotter and was unprepared for it. The delay which it caused made me feel that there was no use in trying to conceal my knowledge."

Mr. W. gives the following account of his attitude toward the experiment: "When I entered the classroom I was not guilty of carrying out the directions. I tried to make my associations quickly and uniformly. Several times a word, generally the name of some object before me, came to my mind before the stimulus word was given by the experimenter, and I pronounced this word, although possibly it had no connection whatever with the word given to me. I did not try to trick the experimenters except when I tried to think of some objects which Mr. L. would be likely to see in the shop. I mentioned some of these objects with the idea that possibly the experimenters would be led to think I knew a little about the series of acts. I had no reason for acting in any other than my ordinary manner."

The basis for our judgment concerning the facts revealed by this experiment is even more satisfactory than that furnished by experiment 1. Indeed the case against Mr. L. was clear before the experiment had been completed.

Subsequently, still further to test the reliability of our results, we repeated these two experiments with subjects who had considerable knowledge of psychology.

Experiment 3.

Experiment 3 is essentially a repetition of experiment 1, but with the substitution of certain words which promised to be more satisfactory than some that occurred in the lists of tables 1 and 2. In each list of thirty words ten significant words were given. The subject, a graduate student in Harvard University, was told to give the associated idea quickly, but to do his best to prevent the experimenters from discovering which of the two boxes he had opened. The test was conducted as the others had been, except that there was no class present in the experiment room.

An examination of the averages, variabilities, and ranges for this experiment as they appear in table 6 indicates that we have a less satisfactory basis for our conclusion than in either

of the previous experiments.[1] There are no indications of the inhibitory influence of the significant words of either list. The greater variability and range of the reaction-times to the mouse-box series might indicate that it had been opened; but, on the other hand, since this series was the first to be given it might reasonably be urged that lack of practice on the part of the subject accounts for the greater variability and range. And so we are left uncertain. As a matter of fact both experimenters finally decided, incorrectly, that the card-box had been opened. But they also decided (and this is of prime importance) that the subject had not given associations that were suggested by the stimulus words. Examination of the reaction words indicated that most of them were suggested either by objects in the room or by earlier words of the series. In the light of this fact the only possible conclusion would be that fear of detection of the truth had caused the subject to refuse to follow the directions which were given. The experiment is inconclusive so far as knowledge of which box had been opened is in question, but it furnishes very interesting and important information concerning the subject's mental content.

TABLE 6

Quantitative Results of Experiment 3

	Results for Mouse-Box List		Results for Card-Box List	
	Irrelevant Words	Significant Words	Irrelevant Words	Significant Words
Mean	1.542″	1.471″	1.324″	1.389″
Mean Var.	.330	.223	.174	.112
Difference in Means,	— 0.071″		+0.068″	
Extremes Short	.805	.768	1.006	1.184
Extremes Long	3.037	1.994	2.340	1.616
Max. Range	2.232		1.334	

Had this subject given us true associations instead of ideas which had come to him before the stimulus word was given, we should almost undoubtedly have obtained results similar to those of experiments 1 and 2.

[1] The detailed results of experiments 3 and 4 are not presented, since they offer little in addition to what appears in the other tables.

Perhaps the most interesting feature of the general results of experiment 3 is the high variability of the mouse-box reaction times. If practice had been excluded by previous training, this would have suggested knowledge of the contents of that box. Although we did not get what we set out to get in this experiment, the results would seem to indicate that no matter what method a subject chooses to conceal his knowledge, he is sure to give the experimenters some clue which if followed up skillfully will reveal the truth.

Experiment 4

Experiment 2 was repeated, as experiment 4, with the list of words reduced to fifty, of which twenty were significant. The list was otherwise changed slightly from the original, by the substitution of certain words. The subjects for experiment 4 were Mr. G., the subject in experiment 3, and another graduate student in Harvard whom we may refer to as Mr. M.

The results of this experiment were definitely positive, for it was clear that Mr. M. instead of Mr. G. had knowledge of the series of acts. This was indicated as strongly by the fact that many of his associations referred to objects in the drawer, as by the inhibitory influence of the significant words and the great variability of their reaction times.

Table 7

Quantitative Results of Experiment 4

	Results for Mr. G.		Results for Mr. M.	
	Irrevelant Words	Significant Words	Irrevelant Words	Significant Words
Mean	1.340″	1.369″	1.853″	2.269″
Mean Var.	.251	.159	.181	.307
Difference in Means	+0.029		+0.416	
Short Extremes	.860	1.063	1.479	1.773
Long Extremes	2.585	1.764	2.278	3.106
Max. Range	1.725		1.627	

Finally, in table 8, we have brought together the mean variabilities and maximum ranges[1] for the reaction times of

[1] It is to be noted that as the maximum range we give the difference between the shortest and the longest reaction-time of the series.

the several experiments. As experiment 3 did not conform to the conditions prescribed for the tests its results have not been included in the general averages of this table.

TABLE 8

General Quantitative Results of Experiments

	Objects or Acts Known to Subject		Objects or Acts Unknown to Subject	
	Mean Variabilities		Mean Variabilities	
	Irrelevant Words	Significant Words	Irrelevant Words	Significant Words
Experiment				
1	.366″	.445″	.449″	.352″
2	.221	.343	.224	.145
3	.330	.223	.174	.112*
4	.181	.307	.251	.159
Average	.256	.365	.308	.219
	Maximum Range		Maximum Range	
Experiment				
1	2.253		1.518	
2	2.562		1.684	
3	2.232		1.334*	
4	1.627		1.725	
Average	2.147″		1.642″	

*Results for Experiment 3 are not included in the averages.

In conclusion we would say that although there are obviously many environmental and subjective factors whose relation to the results of an association reaction-time experiment must be known before the method can be considered reliable, this does not seem to us a fit place to enter upon a discussion of them. It has been our sole purpose in this paper to show that under ordinary conditions and with ordinary skill in experimentation we have succeeded in demonstrating to a class, much to the surprise and satisfaction of its members, that a simple list of associations and their reaction times may reveal intensely interesting facts concerning the content of consciousness.

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STUDIES IN RETARDATION

As Given in the Fatigue Phenomena of the Tapping Test.

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CONTENTS

		Page
1.	Retardation and the Curve of Work. (12 Cases.)	38
2.	The Results as the Product of Retardation.	46
3.	The Method as a Measure of Retardation.	51
4.	Retardation and Depression.	54
5.	Summary and Conclusion.	58

1. Retardation and the Curve of Work

The experimental problems presented by this striking mental symptom are important and numerous. Definition adds little to what is implied in the term itself; in its pure state it is simply *a loss of the promptness of response to the given situation*. It does not necessarily carry with it any mental confusion; thus a long interval may elapse between a question and its answer, but when the answer does occur it may imply as perfect comprehension and insight as would be expected in the normal state. Motor reactions are delayed, ineffective, and give a superficial appearance of extreme difficulty and (usually) hesitation.

While extremely similar conditions occasionally appear in other psychoses, retardation is *par excellence* a symptom of manic-depressive depression, the depressed phase of the Kraepelinian manic-depressive insanity. In this disorder it is perhaps the most familiar objective symptom, and it is only in its relation to this disorder that it need be considered here. Its physiological correlate is unknown, and of its intimate psychological nature much still remains to be cleared up, especially in the analysis of its factors, and the relative part which sensory, associative and motor elements may play in the ultimate product.

The present study proceeds mainly along the lines first suggested by Hoch. Clinical observation indicates that under the continued activities of the waking day the characteristics of the retarded state in circular depressions tend to become less profound, and that these activities effect a change in such

cases far more marked in character and amount than in the case with normal individuals. From this fact the inference is suggested, that if we could standardize the activities of such cases over a certain period, introducing controllable and measurable forms of activity, we should find in the measures thus obtained characteristic deviations from the normal corresponding to the clinically observable changes in the patient's condition. One would thus be afforded at once a quantitative measure for the extent of retardation, as well as a means of detecting its presence and changes in its amount so slight as to escape ordinary clinical observation.

The experimental problem then, is a study of the work curve in retarded individuals, but the various psychological tests that can be applied to the problem of the work-curve are not equally well suited to the purposes in hand. The measures most frequently employed in problems of this nature are those of the ergograph and the addition test. No one will dispute the value of the mass of data which Kraepelin's pupils have accumulated about these measures, which the present studies largely confirm at their points of contact. Certain advantages may perhaps be urged for the present method. The ergograph is strong where the addition test is weak, namely in the objectivity of the results, simplicity of procedure, the control of co-operation, and in the amount of time consumed in the experiment. The addition test is strong where the ergograph is weak, namely in the simplicity of the apparatus, and the detail and minuteness with which it is possible to trace such factors as warming up, reflex inhibition and the *Antriebe*. The tapping test, however, while retaining the objectivity and precision of the ergograph, sacrifices none of the detail and minuteness possessed by the addition test under sufficiently refined conditions of measurement. In the matter of its demands upon co-operation, the tapping test is easily the superior of both. Further, there is probably no motor measure which is less dependent upon muscular factors.

The method and conduct of the experiments is identical with that described in a previous paper.[1] Each individual curve consists of 30″ of continued tapping, evaluated according to number of taps executed in each 5″ interval during the 30″ period. Between each of two 30″ periods of tapping there is a 2′ 30″ pause. Six successive 5″ *intervals* thus constitute a *series* (or individual curve), and five 30″ series with one hand form a *record*. The two records of right and left hand constitute the single *experiment*. There are given the results of two experi-

[1] Normal Performance in the Tapping Test, *American Journal of Psychology*, XIX, 1908, pp. 437-83.

TABLE I

Number of taps executed in 30″ periods. In each experiment the subject executes five 30″ series with one hand, then five series with the other hand. The second experiment follows one week upon the first.

First Experiment (Right Hand Preceding)

Rt. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st Series,	160	185	203	160	151	182	242	154	72	174	202	173	168.3	192
2nd "	164	183	200	165	139	171	225	160	73	164	209	168	164.4	190
3rd "	145	188	209	172	140	179	232	162	75	174	181	173	167.6	194
4th "	153	191	208	175	138	190	236	165	76	191	187	171	172.3	199
5th "	144	188	218	177	137	198	239	164	85	189	204	150	173.9	199
Av.	153.2	187.0	207.6	169.8	141.0	184.0	234.8	161.0	76.2	178.4	196.6	167.0	169.3	195

Lft. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st Series,	148	167	196	179	143	171	213	148	69	192	187	162	162.6	173
2nd "	136	166	196	170	140	156	210	154	80	181	190	149	158.9	172
3rd "	141	167	196	177	140	155	215	152	83	184	177	154	161.0	174
4th "	135	175	192	180	138	158	215	153	79	190	173	151	161.5	172
5th "	149	167	206	166	149	148	215	153	78	190	168	152	162.1	173
Av.	141.8	168.4	197.2	174.4	142.0	157.6	213.6	151.6	77.8	187.4	179.0	153.6	161.2	173.0

TABLE I (Cont.)

Second Experiment (Left Hand Preceding)

Rt. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st Series,	178	197	188	128	144	186	238	156	89	168	202	147	167.2	189
2nd "	188	188	194	131	134	191	234	155	89	179	203	158	168.3	191
3rd "	188	188	198	137	147	195	234	159	93	170	204	144	170.9	195
4th "	184	188	207	139	152	200	233	160	84	185	203	155	173.2	197
5th "	195	191	212	146	150	198	250	156	91	175	200	149	176.4	196
Av.	186.6	190.4	199.8	136.2	145.4	194.0	237.8	157.2	89.2	175.4	202.4	150.6	171.2	194

Lft. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st Series,	152	170	191	144	143	152	206	141	87	177	181	146	156.3	178
2nd "	137	169	189	135	143	151	193	135	92	164	185	137	151.0	175
3rd "	154	170	187	162	147	159	196	140	88	171	191	129	157.4	177
4th "	161	171	193	143	149	164	202	144	85	160	178	165	157.2	176
5th "	152	168	202	144	142	170	211	145	85	161	185	160	158.0	175
Av.	151.2	169.6	192.4	144.6	144.8	159.2	201.6	141.0	87.4	166.6	184.0	147.4	155.8	176

TABLE II

Average number of taps in each 5″ interval of the five thirty second series with either hand in each experiment. Fatigue is shown in the decrease in the number of taps as the later intervals are reached; "reversal" or interserial warming up in the failure of the successive intervals to regularly decrease.

First Experiment (Right Hand Preceding)

Rt. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st interval, 0″-5″	24.0	31.0	38.0	29.4	22.8	30.2	41.4	27.6	12.8	30.2	32.0	27.2	28.7	35.3
2nd " 5″-10″	24.8	32.8	36.0	29.4	23.8	31.6	41.2	27.0	12.8	31.0	34.0	28.2	29.0	33.5
3rd " 10″-15″	25.8	32.0	34.2	28.8	23.2	31.0	39.0	26.6	12.2	30.2	32.6	27.8	28.3	32.3
4th " 15″-20″	26.8	30.6	34.2	28.6	24.0	30.6	39.0	27.0	12.8	29.0	34.2	28.0	28.2	31.5
5th " 20″-25″	25.8	30.8	32.8	27.4	23.6	29.8	38.0	26.4	12.8	29.2	31.8	28.2	27.7	31.0
6th " 25″-30″	26.0	29.8	32.4	26.2	23.6	30.8	36.2	26.4	12.8	28.8	32.0	27.6	27.3	30.7
Index of fatigue *f*	1.08	1.01	.89	.96	1.04	1.02	.93	.97	.99	.99	1.03	1.03	.98	.90

(*f* is the average of the last five intervals divided by the first.) With reference to this measure, Cf. *American Journal of Psychology*, XIX, 1908, pp. 467 *et seq*.

Lft. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st interval, 0″-5″	23.8	29.6	36.8	32.0	23.4	28.2	38.6	26.6	12.8	33.8	31.2	24.6	28.6	32.0
2nd " 5″-10″	24.0	29.4	34.4	30.8	23.6	27.0	37.0	25.4	13.0	32.2	30.6	25.8	27.7	31.1
3rd " 10″-15″	23.8	28.8	32.8	29.8	24.0	26.0	36.0	25.2	12.8	31.4	29.0	26.0	27.1	28.7
4th " 15″-20″	23.4	27.0	30.8	28.6	24.0	26.0	35.0	25.0	13.2	30.6	29.8	25.8	26.4	28.0
5th " 20″-25″	23.8	26.6	31.2	26.8	23.2	25.6	33.4	25.2	13.0	29.8	29.6	25.2	26.9	27.3
6th " 25″-30″	23.0	27.0	31.2	26.4	23.8	24.8	33.6	24.6	13.0	29.6	28.8	26.2	25.7	26.9
f	.99	.94	.87	.89	1.01	.92	.91	.94	1.01	.91	.95	1.06	.93	.89

f	.99	.94	.87	.89	1.01	.92	.91	.94	1.01	.91	.95	1.06	.93	.89

TABLE II (Cont.)

Second Experiment. (Left Hand Preceding)

Rt. Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st interval, 0″-5″	31.2	32.6	37.0	22.4	24.2	33.0	44.0	26.4	14.6	30.0	32.8	24.0	29.5	35.0
2nd " 5″-10″	32.0	33.0	34.4	22.2	24.0	33.0	41.4	26.2	14.6	30.0	33.6	24.6	29.1	33.2
3rd " 10″-15″	31.4	31.8	33.4	23.4	24.2	32.6	39.6	26.4	15.0	29.8	33.4	25.2	28.8	32.2
4th " 15″-20″	31.4	31.8	32.2	22.2	24.4	32.4	38.8	26.4	15.0	29.8	34.2	25.6	28.3	31.6
5th " 20″-25″	30.2	31.0	31.8	23.4	23.8	31.4	37.0	25.8	15.0	28.8	33.6	25.4	27.8	31.4
6th " 25″-30″	30.4	30.2	31.0	22.6	24.0	31.6	37.0	26.0	15.0	28.0	34.8	26.0	27.6	30.7
f	.99	.97	.88	1.02	1.00	.98	.88	.99	1.02	.98	1.03	1.06	.96	.91

Lft Hd.

Depressions. Case	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Average	Average of 10 Normal Subjects
1st interval, 0″-5″	26.0	30.0	35.0	24.6	22.8	28.2	35.2	25.2	14.0	29.8	31.4	23.2	27.1	32.3
2nd " 5″-10″	26.0	29.6	34.4	24.2	25.0	27.2	34.4	24.2	14.6	28.6	32.4	24.4	26.8	30.8
3rd " 10″-15″	25.6	28.6	32.0	24.4	24.0	26.6	34.2	23.2	14.4	28.4	30.8	24.4	27.0	29.3
4th " 15″-20″	24.8	27.6	31.2	24.4	24.4	26.4	33.2	23.0	14.6	27.6	29.6	25.2	25.7	28.3
5th " 20″-25″	24.6	27.0	30.0	23.6	24.2	25.6	32.6	22.6	14.8	26.6	30.8	25.0	25.2	28.1
6th " 25″-30	24.2	26.8	29.8	24.4	24.4	25.2	32.0	22.8	15.0	25.6	29.0	25.2	25.0	27.5
f	.96	.93	.90	.98	1.07	.93	.94	.92	1.05	.92	.97	1.07	.96	.89

ments upon each of twelve subjects. *In the first experiment the right hand precedes, in the second the left hand precedes.*

The statistical aspect of the results is summed up in the accompanying tables. Table I gives the results in terms of the total number of taps in each of the five 30″ series in each experiment with each hand. In this table the decreased total efficiency of the retarded records as compared to the normal average is the principal thing to note. In Table II the figures are given according to the averages of all the series for each 5″ interval, in order to convey an idea of the fatigue phenomena which the series show. It will be noted that the series for the depressed subjects, while slower in rate, show very much less fatigue than does the normal average. In these two facts lie the principal deviations of the depressed subjects from the normal.

It will be observed that in the averages for the intervals (Table II) the first interval is often surpassed in rate by a later interval, which is not the case in normal subjects. To this characteristic phenomenon we shall apply the name of *reversal*. It is an in*tra*-serial warming up, warming up within the individual series, as distinguished from the in*ter*-serial warming up, warming up between the series, discussed at some length previously.[1] As a marked illustration of this *reversal* may be quoted the figures from Table II of Case I's right hand in the first experiment; in the first five seconds he averages 24.0 taps, in the second five 24.8 taps, in the third 25.8, in the fourth 26.8, in the fifth and sixth intervals 25.8 and 26.0. That is, there is regular "reversal" to the fourth interval, the opposite of the normal curve. Case III on the other hand shows no "reversal" at all.

The index of right handedness[2] shows an analogous phenomenon, to which is applied the name *transference*. In the normal individuals it was noted that the index tended to no regular fluctuations according as the right or left hand preceded. Here, however, many of the cases show a very pronounced tendency for the index to be lower (*i. e.* a relative superiority of the right hand) in the second experiment, when the right hand follows. The indices in the depressed cases are given in the table on the next page.

The indices show that in most cases the work with the preceding hand *transfers* its warming up effect to the work with the following hand.

Cases I-X are under the diagnosis of manic-depressive depression. In Case XI the depressive picture is complicated with

[1] *Am. J. Psych.*, XIX, 1908, pp. 446-47, 457-61.
[2] *Ibid.*, pp. 446, 454-56.

Index of Right-Handedness, (Lft. Hd.÷Rt. Hd.)

Case..	I	II	III	IV	V	VI	VII	VIII
First Experiment, Right Hand Preceding	.93	.90	.95	1.03	1.01	.96	.91	.98
Second Experiment, Left Hand Preceding	.81	.89	.96	1.06	.99	.82	.85	.90

Case..	IX	X	XI	XII	Average	Average of 10 Normal Subjects
First Experiment, Right Hand Preceding	1.02	1.05	.91	.92	.95	.89
Second Experiment, Left Hand Preceding	.98	.95	.93	.95	.91	.91

traumatic traits, in Case XII with psychasthenic symptoms. These two cases do not therefore present a manic-depressive picture sufficiently unequivocal to make it advisable to include them in the averages of the other ten. Their experimental correspondence with them, however, justifies their separate quotation. In addition, experiments have been made upon some thirty other pathological subjects, representing various disease groups.

To any one familiar with cases of this type, it will be scarcely necessary to say that, although presented under one and the same diagnosis they really afford very diversified clinical pictures. These individuals are in all respects much more different from one another than the ten normal subjects to whom we perforce compare them. These individual differences have naturally a great influence on the character of the results we obtain from them. Many striking peculiarities in the individual records would become much more intelligible if these differences were made clear. To do this, however, would expand the paper to more than three times its present proportions, and even so would intrude much matter in a form significant only to the clinician who could read between the lines of the histories and physicians' notes. It has seemed preferable, therefore, to dispense for the present with any detailed treatment in connection with the individual pictures, which are best presented primarily from the clinical viewpoint. It may be said at the outset, however, that no subject figures in the present data whose co-operation would not have been regarded as entirely trustworthy in a normal subject.

2. THE RESULTS AS THE PRODUCT OF RETARDATION

In gross rate there is an entirely fundamental difference in favor of the normal individuals. Taking the normal standards for right and left hands as 194 and 175, those for the depressed subjects become 170 and 159. It is thus also seen that in the depressed subjects there is rather less difference between the right and left hands than in the normal subjects. In the relationship of the five successive series to each other, the groups show no difference worth mentioning. In both normal and depressed subjects inter-serial warming up is noticed in the right hand, not in the left. As regards individual differences in the two groups, these are distinctly greater in the depressed subjects. The m. v. of the average "total efficiency" figures for the normal and depressed subjects in each experiment, as well as for the average indices of fatigue, or *f*'s, are as follows:

	M. V. of Gross Rates				M. V. of *f*'s	
	R1	R2	L1	L2	R	L
Normal	14	18	14	16	.038	.030
Depressed	29	31	27	22	.043	.041

The depressed individuals, then, differ more among themselves than do the normal individuals. Since this was true also of age, and of other characters irrelevant to the depression, it is not possible to say how far this greater variability is in direct relation to the depression; to some extent, it probably is so.[1]

In the average gross rate for the normal individuals, the preceding hand is the more favored; in the depressed individuals, on the contrary, it is the following hand which is the more favored. The final figures expressing this relation are as follows:

	R1	R2	L1	L2
Normal	195	194	173	176
Depressed	169	171	161	156

In R1 and L1 the left hand follows, in R2 and L2 the right hand follows. Each hand is better when it follows than when it precedes in the depressed subjects. Each hand is better when it precedes than when it follows in the normal subjects. We noted this tendency of the following hand to be the more favored in many of the depressed subjects individually, under the name of "transference"[2] and we now observe that this tendency is sufficient to be reflected in the averages.

[1] The same observation is made by Hutt (*Ps. Arb.* V., p. 354), the women being less variable than the men. It may be noted that the left hand is here less variable than the right, as was also found in the normal group.

[2] *Cf.* the figures for the index of right handedness, p. 45

Finally, there is the fundamental difference between the two groups in the matter of their relative immunity to fatigue within the individual series. In considering the individual cases we meet time and time again with fatigue phenomena absolutely foreign to what has been observed in normal individuals. Not only is the *f* in the depressed cases abnormally high, differing from the normal *f* considerably beyond the limits of the probable error, but there are noted in many indvidual cases fundamental alterations in the shape of the fatigue curve itself. In these cases the curve of work does not begin at its maximum efficiency with gradual loss by fatigue, as in normal subjects, but there is a prolonged period through which the performance actually increases to a level above its initial efficiency. The most marked instances of this phenomenon are to be found in Cases I, IV, and XII. We have here a set of favorable influences operating on the curve of work which are sufficiently potent to more than counterbalance the fatigue losses which normally have the preponderating influence. The degree to which these influences operate naturally varies with the different subjects. In the most extreme series with Case XII the increase in rate was continual throughout the 30 seconds, and it is impossible to say how much longer the gain would have continued before fatigue overbalanced it; in Case II, on the other hand, the gain was apparent only in the right hand and then not further than the second or third interval. In other cases, as VIII and X, these favorable influences are not sufficiently strong to make the later intervals actually faster than the earlier, and their effects are seen merely in a raising of the *f* towards the limit 1.00, which is surpassed in such cases as XII or V.

Only with great caution, however, is an *f* below 1.00 to be accepted as evidence of retardation in the absence of reversals. An *f* above 1.00 cannot occur without reversal; but an *f* below 1.00 can occur either with reversal, or merely a very slow fatigue loss. This latter condition may mean, as above indicated, a true retardation, but it may be also the result of indifferent co-operation, and we usually have no evidence which. One objective criterion of co-operation we occasionally do have: if during the course of the tapping the energy directed to this work "overflows," as it were, into other motor pathways, and the subject shows generalized body movements in addition to the tapping, co-operation can ordinarily be accepted without further question; but entirely trustworthy co-operation occurs without this being observed at all. Beyond an unmistakable lowering of gross rate, the only justifiable criteria of retardation in these experiments are reversal and transference. These are essentially unaffected by differences in co-operativeness.

Either reversal, transference, or both, is observed in all the twelve preceding subjects save Case III. This case gave the appearance of some retardation; there is certainly more ground for surprise at not finding evidences of it in the results than there would have been at finding them. It remains exceptional among the present observations.

Turning attention now to the more general aspect of the results as given in the above special and particular manifestations, it is evident that the common factors in all the anomalies noted in the depressed records are a lowered gross efficiency of performance and an increased responsiveness to the favorable influences operative upon the curve of work. The lowered efficiency of performance is manifested in the decreased gross rate, roughly to 89% of the normal. The increased responsiveness to favorable influences operative upon the curve of work is manifested in the greater efficiency of the following hand as such (transference) and in the phenomenon of intra-serial warming up (reversal).

The general slowing up of all responses which forms so prominent a feature in a large number of depressions, is described under the generic term of *retardation;* and in the present results we are afforded a quantitative index of certain phases of this symptom. Retardation is to be regarded as the most important factor in the lowered gross rates in these cases, nor should we in its absence observe the increased responsiveness to favorable influences which plays so great a part in the present experiments.

With regard to the exaggerated transference effects as a phase of the retardation may be cited the observations of Hoch.[1] These experiments were made on the ergograph, with exceptionally careful technique, and consist each of three "exhaustion" curves executed at intervals of 15 minutes. For comparison with the present experiments, therefore, each curve corresponds best with a record with one hand and the 15 minutes to the 2′ 30″ pause between the hands; save that 15′ was known to be the *günstige Pause* for the ergograph, while we do not know the relation of the 2′ 30″ pause to the tapping test.

The retardation was considered to be given in the fact that the depressed cases showed an increase in the number of times the weight was lifted (*Hubzahl*) throughout the three curves on a single day, though the *Hubzahl* shows no practice increase from day to day. The increased efficiency of performance of the following curves over the preceding ones is entirely parallel to the more favorable position of the following hand noted

[1]On Certain Studies with the Ergogaph. *Journal Nerv. Ment. Disease,* XXVIII (1901), pp. 620-628.

in the present subjects. In Hoch's terminology, we may speak of the preceding hand overcoming certain "resistances," which brings about a greater efficiency in the later series with the following hand. Under this interpretation the "resistances" overcome must be of a general nature since the one hand benefits from the work of the other. The only difficulty in the analogy between the two experiments is, why do not the earlier series bring about greater efficiency in the later series of the same hand, *i. e.*, an exaggeration of inter-serial warming-up? We do not obtain this in any significant degree save in certain other experiments with Cases IV and XII, where there is exceptional overcoming of retardation; in this respect the normal and depressed subjects are practically on an equal footing. Still, when the fundamental differences in the methods are taken into account, this discrepancy cannot be considered a very formidable one. The same general process is at work in both sets of results.

The phenomenon of intra-serial warming up, *i. e.*, the gain in the later intervals over the earlier (reversal), is more closely paralleled in the experiments of Hutt.[1] The object of inquiry in these experiments is practically identical with the present, Hutt making use of the addition test. The depressions are 15 in number, 5 men and 10 women. The properties of the addition test under these experimental conditions in normal subjects are fairly well known. When the ten minutes of work are continuous the number of additions performed in the second 5′ is regularly less than that of the first 5′. If, on the other hand, a 5′ pause is inserted between the two halves of the experiment, the second 5′ is regularly better than the first 5′. Hutt's depressions, like the present ones, showed a lowered efficiency of performance; but whether a pause was inserted or not, the second 5′ showed an unequivocal tendency to be better than the first 5′ The improvement in the second 5′ without the pause corresponds to the improvement in the later intervals over the earlier (intra-serial warming up, reversal) noted in the present experiments. With regard to the days with the pause, the situation is complicated. Normal individuals gain in the second 5′ with the pause, but, according to the normal results given by Hutt from the researches of Plaut and of Rehm (p. 356) *they do not gain so much as the depressed individuals when these latter gain at all.* With considerable frequency occur depressed individuals who lose after the pause, and statistically there is very wide variation in Hutt's Table I, Column 3, (p. 355), between those who gain after the pause and those who lose. All loss after the pause is abnormal, and those

[1]Rechenversuche bei Manisch-Depressiven. *Ps. Arb.* V, 3 (1908), pp. 338-370.

who gain after the pause very frequently gain more than the normal. This abnormal gain after the pause is another aspect of the abnormal gain from 15′ rest to 15′ rest noted in the ergographic experiments of Hoch, and probably also of the hand to hand "transference" gains in the present experiments. Those who lose after the pause suggest the "more pronounced" case mentioned by Hoch (Ps. Bull. I, 255), in which both "warming up" and practice were absent.

For their behavior with respect to the pause, Hutt divides his material into four groups. (a) Those who lose without and gain with the pause. This is the normal relationship, but quantitatively, *the loss in the depressions is less, and the gain is more.* (b) Those who gain with and without the pause (by far the most numerous). (c) Those who gain without and lose with the pause. (d) Those who lose both with and without the pause. Though small, these last two groups are of some interest from the interpretational point of view. Be the experimental approach the ergograph, addition test, or tapping test, the interpretation "overcoming of initial resistances through the warming up effects of continued effort" will cover all cases in which the work curve of depressed subjects (a) loses less than the normal loses, (b) gains more than the normal gains, (c) gains when the normal loses. The remaining cases are those who both gain when the normal should lose (without the pause), and lose when the normal should gain (with the pause), and those who lose in both instances. For the loss after the pause Hutt (p. 360) suggests that the cessation from the effort for a sufficiently long period allows the retardation to again establish itself. (*Cf.* also Hoch, p. 624.) The resistances, now added to the fatigue induced by the previous 5′, suffice to make the second 5′ poorer than the first. There are only two cases who, losing with the pause, also lose without it. In neither of these cases does Hutt lay any special stress on the loss without the pause, attributing it to chance *Antriebswirkungen* in the first 5′; but this interpretation cannot be well reconciled with the loss after the pause, for which he invokes in the second case "rasches Schwinden der Arbeitsbereitschaft durch *Wiederauftreten*[1] der Hemmungen in der Pause." *Hemmungen* cannot *wieder auftreten* which have not been already *beseitigt*, and the experimental records show no evidence of any *Beseitigung*. Rather do we have here a complete failure of response to any "warming up" influences. "In der Tat," Hutt continues, "bot auch dieser Fall eine sehr stark ausgeprägte Denk- und Willenshemmung dar."[2]

[1]The italics are mine. F. L. W.
[2]*Cf.* p. 53.

But whatever may be the correct interpretation of such exceptional cases, the fact that the same general result is reached in these independent investigations, by as many different modes of experimental approach, points strongly in favor of the similar interpretations suggested in each case. It is interesting to note how closely the phraseology of Hutt parallels that of Hoch, of whose work Hutt seems to have been unaware. He writes, "Wie uns scheint, vermag hier die klinische Erfahrung einen Fingerzeig zu geben, in welcher Richtung etwa die Erklärung für das abweichende Verhalten unserer Kranken zu suchen sei. Die alltägliche Betrachtung lehrt uns, dass sie vielfach Hemmungen unterliegen, die durch eine längere Beschäftigung mit ihnen allmählich an Stärke abnehmen. Gilt dies zunächst für die depressiven Kranken, so tritt uns bei den manischen eine ganz ähnliche Erscheinung in der Erfah-rung entgegen, dass sich bei ihnen die Erregung durch dauernde motorische Betätigung in sehr auffallender Weise zu steigern pflegt. Uebertragen wir diese Gesichtspunkte auf die hier vorliegenden Versuche, so würde uns ihr Ausfall unter der Voraussetzung verständlich erscheinen, dass durch die fortlaufende Arbeit selbst Hemmungen beseitigt werden, die den Ablauf der geistigen Arbeit zunächst verlangsamt haben. Daraus würde sich die geringe Durchschnittsleistung unserer Kranken in den ersten 5 Minuten ihrer Arbeitszeit und das Anwachsen der Leistung in den zweiten 5 Minuten an den pausenlosen Tagen befriedigend erklären. Die bei Gesunden hier überwiegenden Ermüdungserscheinungen würden bei ihnen überreichlich wieder ausgeglichen werden, dass sie im Laufe der Arbeit Hemmungen überwinden, die ursprünglich die Leistungsfähigkeit weit unter die Norm herabdrückten."

So far as the depressions are concerned[1], these remarks apply equally well to the above mentioned experiments of Hoch and those of the writer. The final indication seems to be, that the curve of continued work shows in retarded patients an abnormality characterized by a low efficiency of initial performance, which the overcoming of the retardation through the activity of the work either (a) raises to a higher level, or (b) maintains at or near the initial level longer than would normally be the case.

3. The Method as a Measure of Retardation

Inasmuch as the alterations in the work curve observed in

[1] Hutt's manic cases (8 in number) are distributed among the first three groups of his depressions, though he does not give them quite the same interpretation. In the tapping test the manic performance is the absolute antithesis of the depressed. This apparent contradiction will be discussed under a separate treatment of the manic cases.

retarded individuals are of the same general nature independently of the class of tests we employ, the most reliable results are probably to be expected from the test which is intrinsically the most accurate psychological measure. The advantages of the tapping test in this respect the writer has already endeavored to point out. We have now to inquire to what extent the alterations in the fatigue curve may afford a quantitative measure of individual or personal differences in the extent of retardation. It is necessary to consider two aspects of the curves; first the amount of lowering of gross efficiency; second, the transference and resistance to fatigue, or intra-serial warming up. The first factor would indicate the absolute amount of retardation, the second, how far this retardation tended to be overcome. For comparisons between different individuals, the method is very limited indeed. It is difficult to say whether A is more retarded than B from a comparison of their gross rates; the fundamental individual differences are too great. (*Cf.* Hutt, p. 353.) Nor does the amount of intra-serial warming up or "reversal" afford a reliable index of whether one individual is more retarded than another, *e. g.*, these phenomena were most marked in Subject XII, in whom retardation was hardly, if at all, clinically evident.

A much more practical question is whether the method affords a means of measuring changes in the degree of retardation and thus reflecting changes in the condition of the same individual. Its behavior in this respect is a much more constant one. Increased gross rate regularly accompanies improvement in condition, decreased gross rate a drop in the condition. It is interesting to note that it does this with rather more regularity than seems to be the case in normal individuals. Through the thirty experiments reported in a previous paper,[1] the writer could detect no relationship between gross rate and subjective condition, and this result was entirely confirmed by unsystematic inquiries made in the case of Subject II. Among the present subjects, in Case III alone is the clinical change opposite to that of the gross rate. In Case VII, the gross rate does not reflect a slight clinical change. In Case V there is neither clinical nor experimental change. Changes in gross rate occur in the absence of *positive* clinical observations in Cases I, VI and XI. More careful and extended observations on Cases VIII and XII as well as Cases II, IV, IX and X reflect a correspondence with entire clearness.

Knowing the gross rate of a depressed subject in the normal state, the performance in the depressed condition should afford

[1] *Loc. cit.*, pp. 480-1.

an indication of the extent of retardation as well as its presence, but as these experiments are made we do not know the normal rate. Only as the rate actually found changes with the patient's condition is any evidence of retardation found in the rate alone. Owing to this limitation, we can be much surer of the presence of retardation when we find the characteristic alteration in the shape of fatigue curve (intra-serial warming up, reversals and transference) brought about by the overcoming of its resistances. The relation which is indicated beween the gross rate and the warming up phenomena is complex, but consistent. Generally speaking, the lowered gross rate indicates a condition of retardation. The warming up phenomena indicate the extent to which the continued activity of the test tends to overcome this retardation, *i. e.*, the responsiveness of the the individual to warming up influences. We shall not, of course, expect abnormal warming up phenomena in the absence of retardation; the hypothesis that resistances are overcome postulates resistances present to be overcome. The important thing to remember is that they may be present without being overcome; and that we may have considerable retardation without the slighest trace of warming up effects. Indeed, there is evidence that these cases, in which we may obtain a fatigue curve quite resembling the normal in form, may present rather the profounder degree of retardation. A slight retardation then, will show a somewhat lowered gross rate and decreased susceptibility to fatigue, with reversal perhaps in the second interval (*Cf.* Case II); when the retardation becomes deeper, the gross rate is further lowered, and the immunity to fatigue is more marked, with reversals in the later, even up to the final intervals; with yet more profound retardation the warming up effect fails, and we obtain in the later intervals a loss approaching the normal. Hutt's Figure III probably illustrates these phases better than do the here quoted results. Considering of course, only the results without the pause, we obtain first the three depressions who lose during the second 5′, just as normal individuals would do (Group a), then a group of eight depressions gradually gaining more and more during the second 5′ (Group b), then another group of three depressions who, while they still gain during the second 5′, yet lose with the pause, showing a more profound degree of retardation than the preceding, (Group c), and finally two depressions (3 records) who again lose during the second 5′, (Group d), just as did the group (a) at the other extreme. The present observations, when considered in connection with the clinical data, are entirely confirmatory of Hutt's findings on this point, and certain individual cases, notably IV and XII, illustrate the matter rather more strikingly, owing to the greater

precision of the method. Such interpretations, however, have no relation to the comparison of the depth of depression in different individuals, applying only to the different conditions of the same individual.

4. Retardation and Depression

With this result the strictly experimental portion of our inquiry ends, but in closing, a few words may be devoted to the psychological relations of retardation to the general symptoms of the manic-depressive disorders that seem to maintain the best correspondence with the observed facts of clinical experience. We shall make a grave mistake if we start out with the idea of finding any single formula that will express the relationship of depression and retardation in all manic-depressive states. We shall have to distinguish at least three forms in which retardation may occur, as follows:

1. Cases in which the depression seems to be the fundamental thing and such retardation as is clinically observed can be amply accounted for as secondary to the depression. Case VIII is an excellent example of this type.

2. Cases in which the retardation is fundamental, even to the extent of swallowing up the depression. The present observations contain no really classical case of this type; its best representatives are probably Cases IV and VI.

3. Cases in which the retardation is independent of the emotional condition, including other conditions than manic-depressive insanity.

In the first group we have a class of cases that may be most instructively compared with the reactions in our own depressed moods. Here, as there, the essential of the state is its emotional tone. Of the physiology of the emotions we know little enough, and still less of their pathology; but that emotional states are the product of physiological adjustments of some sort, and do not exist in the absence of these adjustments, few will be found to deny. Every physiological adjustment of the organism carries with it its own essential emotional tone. Such readjustments are, of course, brought about in various ways. In the emotional sphere, it is usually central causes which are the immediately determining factors, though the influence of organic processes, such as digestion, is, of course, far from negligible. The same physiological adjustment as is brought about through a severe mental shock, may be brought about through a specific disease process, just as bodily secretions are influenced alike by mental causes, and by the action of drugs. In normal life, these adjustments occur as reflex responses to the various situations that we meet, and the emotional state is given immediately in the bodily state ensuing

upon the response, because this itself constitutes the emotional state. In the psychosis, a certain adjustment is maintained through a pathological condition, and the world is seen through dark glasses.

The former condition results in the transitory depressions which many of us know only too well; the latter, in the fundamental depressions of the psychoses. The appearances which the two conditions present to ordinary observation are really very similar. The "feeling of inadequacy" is beyond question a concomitant of our own depressed moods, and the same must be said of retardation in so far as it is the direct offspring of inadequacy. The changes in speech and gait are especially illustrative in this respect. In sensitive and unstable individuals more serious manifestations may occur, as delusional interpretations of trivial occurrences, and the contemplation of suicide. But the basic difference between the two states remains, in that the former is the product of external, mental causes, and would disappear upon the removal of these causes,[1] while the latter is an internal organic manifestation that responds little, if at all, to mental influences, and disappears when the disease process ceases to be active. It is also true that there is less clouding in the normal depressions, but we must remember that the delusional content in the manic-depressive depressions is often secondary, and results from the delusional coloring, by the depression, of insignificant happenings, or through a feeble attempt to account for the existence of the depression. Moreover, the very group of psychoses in which confusion is the most prominent, and would have the best title to be assigned a fundamental value, are precisely the conditions in which *hallucinosis* is also the most prominent, and this upon a specifically toxic history; namely, the acute exhaustion and the drug psychoses. Here the delusions may be largely secondary to the hallucinations. Altogether, no great stress can be laid on delusions as a primary symptom in manic-depressive insanity, and a considerable number of cases will fail of correct interpretation without the full recognition of the fundamental character of depression and the multiplicity of symptoms, including retardation, which can be secondary to it, as well in normal life, as in the psychoses.

The second group is not so easy to interpret from the standpoint of our normal reactions, but we are forced into making such a group on account of the great disproportion that often exists between the retardation and the depression. We cannot

[1] As in Banquo's reply to Macduff on learning of the murder of Duncan,

> "Dear Duff, I prithee, contradict thyself,
> And say it is not so."

satisfactorily account for all phenomena of retardation on the basis of its being secondary to depression. Indeed, some points of view regard the retardation as the one fundamental quality, and the depression as secondary to it. It may be questioned, however, whether such a formula does not involve a conception of the term retardation more limited and specific than is yet justified in clinical knowledge. If an individual becomes depressed at finding himself primarily retarded, clearly the brain-processes which give the consciousness of the retardation do not share to any great extent in the retardation. It must be obvious that a fundamental and diffused retardation must in time result in a general dulling of mental faculties, in which the emotions share. This would give us ultimately a condition resembling apathy rather than depression, and is one that we not infrequently see. In its profounder manifestations, every trace of affective life may be completely engulfed in the retardation. Characteristic motor symptoms may ensue, such as negativism, stereotypies, catalepsy and the like, which often give the case an extremely præcox-like appearance. This, and not affective depression, is the logical result of a generalized retardation.[1] If the reader will pardon a somewhat loose employment of the adjective, a "peripheral" retardation is the normal psychomotor result of a depression (at least in younger individuals); but that a depression might be secondary to a retardation, the retardation, while it might involve some element of a pure thinking disorder, would usually be one involving only relatively low levels of psychomotor activity, a retardation *die sich im Rückenmark abspielt*, to borrow a phrase of Jung's.

But while maintaining the existence of retardations of the above general type and their special influence on the emotional tone, this specialized type of retardation may after all be the best conception of those cases in which the retardation appears to preserve an absolute independence of the affective life. It is very difficult to understand how a fundamental, generalized retardation could have for its affective result any other condi-

[1] Here, again, we have a not inept parallel in normal life. The fatigue following physical exertion is accompanied by a condition not dissimilar to retardation, in which the emotional life shares to a peculiar extent. If while we are in such a condition we are met by a situation of considerable affective value, be it of an elevating or depressing nature, it is quite noticeable that the affect is, under these conditions, considerably less than when we are fresh and unwearied. There is teleology in this, for the promptness, decision and general efficiency of the outward response to a situation tends to be inversely related to the affective power of the situation, and it is precisely in situations likely to arise under such circumstances that a relative efficiency of the reaction may be most urgently demanded.

tion than apathy, and if we are to consider retardation as *secondary* to all the affective states with which it is clinically associated, we shall indeed make of it a psychiatrical *fils de vingt pères*. Kraepelin analyzes the mixed states into variations of three fundamental symptoms, associative retardation or thinking disorder (*Denkhemmung*), motor retardation (*Willenshemmung*), depression (*Verstimmung*), and their opposites. The mixed states are given us in the different combinations of these traits. Inasmuch as in the regular pictures of manic-depressive depression the three run a largely parallel course, the writer has employed the term *retardation* to cover both the first and second of these symptoms; but in the mixed states we have to keep them separate, as we see them occurring independently, and must assign to each a fundamental value. We may find a flight of ideas and either a motor excitement or retardation, combined with either an emotional exhilaration or a depression; we may find a slowness in thinking (thinking disorder) and either motor activity or retardation, combined with either emotional depression or exhilaration. In these states it is difficult to conceive of any one symptom being secondary to another, because they are all so independent of each other. A psychomotor retardation is not what we should expect in the presence of euphoria (manic stupor), and that it does so occur argues its fundamental quality; in like manner do the cases of difficulty in thinking (thinking disorder), emotional depression, but motor activity (*Depressive Erregung*), and those of flight of ideas, emotional exhilaration, but motor quiescence, argue a retardation in the motor sphere independent and distinct from the intellectual as well as the emotional activities. This is the retardation, "*die sich im Rückenmark abspielt.*"

In sum, the immediate relation between retardation and depression may be threefold. A fundamental depression may give rise to a secondary inadequacy or retardation; a fundamental retardation not involving the higher psychic centres may conceivably give rise to a secondary inadequacy and depression. Both symptoms may appear in their fundamental quality, each presenting its special picture independently of the presence and character of the other. The experimental criteria of retardation that have been adduced do not depend for their existence upon phenomena of consciousness; they are merely special aspects of what may be observed under conditionsin which consciousness is experimentally out of the question, as in the decerebrate frog. Our conception of retardation must, therefore, be more objective than something exclusively secondary and dependent upon the mood; the entire phenomena of retardation are the most easily comprehensible from the physiological point of view.

5. Summary and Conclusion

The phenomena of retardation are here studied according as they affect the curve of continued work. The form of experiment is the "tapping test," each individual curve consisting of 30″ continued tapping, evaluated according to the number of taps made in the successive 5″ intervals of the entire 30″ period. The alterations of the work curve that appear especially associated with retardation are evident not only in the decreased amount of work performed (found in the lowered average efficiency of the group studied) but also in the improvement of the work under conditions in which normal individuals would show a fatigue loss or no characteristic change (*reversal* and *transference*). Among the manic-depressive depressions distinct phenomena of retardation, shown mainly in *reversals*, appear in Case I; in Case II they are less prominent; in Case III they are absent. In Cases IV and V they are again quite characteristic, in Case VI less so. In Case VII they are again absent, but there is a trace of *transference*. Transference is especially prominent in Cases VIII, IX and X. Cases XI and XII show especially prominent phenomena of reversal; in Case XII they are the most clear-cut observed. It is not made out whether reversal and transference represent different types of retardation; they occur largely independently, but simultaneously in Cases X and XII. These results, representing as they do increased responsiveness to the warming up effects of continued effort, are paralleled in the observations of Hoch with the ergograph and Hutt with the addition test. The rate of tapping tends to increase with improvement in condition and to decrease with a drop in condition; but the phenomena of reversal and transference may be swallowed up in a very deep retardation, appear as the retardation becomes less profound, and finally disappear as the retardation itself disappears. Kraepelin builds the manic-depressive depression upon three fundamental symptoms of *Denkhemmung*, *Willenshemmung*, and *Verstimmung*. The term *retardation* includes the first two of these; but the present phenomena are presumably the product of *Willenshemmung* plus *Denkhemmung* or *Willenshemmung* alone. In certain cases *Willenshemmung* and *Denkhemmung* may be secondary to *Verstimmung;* and in other cases *Verstimmung* may be secondary to the other two. All three, however, may and probably do have a fundamental and independent existence, especially in the mixed states. The "feeling of inadequacy" is itself never fundamental, but always secondary to either retardation or depression; it forms a bridge between the two. Whatever its origin, the retardation measured in the present experiments is

to be regarded as the product of immediately physiological conditions.

That free use has been made of the hospital records and physicians' notes in the interpretation of the individual cases, is scarcely necessary to add. The writer is also under obligations to the physicians of Boston Insane Hospital, through whom access was obtained to Cases IX and X. Special acknowledgments are due to the writer's colleagues in the McLean Hospital, Dr. E. Stanley Abbott and Dr. Frederic H. Packard. Such clinical suggestiveness as these studies may possess is largely due to them.

CHAPTERS FOR A BIOLOGICAL-EMPIRICAL PSYCHOLOGY

Nihil in Mente nisi prius in Mundo

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INTRODUCTION

Rectification of frontier between science and philosophy; Penultimate and Ultimate Philosophies, respective aims and subject-matter. Penultimate "explanations" merely re-statements in successively simpler terms; their advantage, progressive shifting of difficulties where they belong (to Ultimate Philosophy). Biological and psychological, wrongly identified with philosophical problem. Biological psychology and existing systems; its predominantly genetic character, utilization of introspection, exactitude, empiricism; it subsums under Penultimate Philosophy and conforms to Penultimate Logic. Consciousness no peculiar mystery, only one phase of the All-Mystery; its subordinate place in a penultimate philosophy of the cosmos, apparent nature and *modus operandi*, survival value. Foundation assumption of scientific psychology, a rigid correspondence of vividness of consciousness with amplitude, of quality of consciousness with length, of dynamic-inseparable wave. The psychologic problem, tracing of evolution of "mind," but the mystery not that of mind but of consciousness. Anomalous electrical conductivity of carbon. Universality of law of conservation of energy not impugned by any psychic fact.

Clear ideas on psychology are first possible after sharpest distinction between matter of *psychology* and matter of *philosophy;* more precisely, between *Penultimate*-philosophic and *Ultimate*-philosophic questions. Penultimate Philosophy is not concerned with noumenal problems, or with "spiritual" or "moral" meaning for the universe. Such questions are, or may be, legitimate for Ultimate Philosophy. Penultimate Philosophy has a different task; to rear a superstructure, our "elements" *being given*. For penultimate purposes, the ultimate nature of these last may be anything,—God, corpuscles, mental figments, energy turned other side to. In this philosophy, "substance" and "energy" denote merely quality-complexes *absolutely definite and constant* within the limits (temporal and spatial) of our experience with them; and "carbon" and "hydrogen" could as well be replaced by C and H, by algebraic symbols for unknown quantities, or by paragraphs enumerative of their known properties. Scientific (and no less common) language is thus stenographic, cryptographic,

hieroglyphic, algebraic, symbolic; everywhere expressive of sums total of sensible (and deducible) properties, nothing more. The final values of our symbols must come from Ultimate Philosophy. But for penultimate purposes, the language of substance-energy is all-sufficient, those purposes being fulfilled when problems are explained in penultimate terms, terms of "matter" and "motion." True, this "explanation" is no explanation at all, but merely restatement of fact in simpler terms, a tracing back of the chain of "continuity" up to, and reformulation of problem in terms of, some earlier link; a *resolution* as distinguished from a *solution*, as in algebra we resolve one equation into another with roots differing by any assignable quantity without getting nearer the values of the roots,—in short, it is a *penultimate* solution. But such resolutions have great advantage; they *progressively shift our difficulties where they belong*, on the *jenseits* of substance, to Ultimate Philosophy.

But it is no less true that *all* the realm of substance-energy lies on the hither side of the resurveyed scientific-philosophic (penultimate-ultimate) boundary. Some will still have "life" and "mind" beyond the jurisdiction of science, but this paper aims to show that these subjects fall within that jurisdiction, and that the biologic and psychologic have been wrongly identified with the philosophic problem.

Each makes his psychology (also his philosophy) in the loneliness of his own mind, and offers it to his fellows to try on. So is offered this attempt of a biologist weary of the stones proffered when in the psychological proletariat he cried unto the Chairs for bread, to bake on his own account. This psychology differs from existing systems in several respects. In conception, methodology, exegesis, it is predominantly biological; mental phenomena are subject-matter of a department of dynamic biology, "mind" is an evolution integration-product. The human (no less the existent avian, the existent reptilian) mind is but the latest ephemeral term in the integration series, and nothing is to be accepted as existing in it[1] which could not have evolved into it. Given a problem in human mind, biological psychology asks: How could such condition have come about, what must have been its tributary

[1] One qualification only: If as the outcome (at the end, not at the beginning) of investigation, some irreducible residuum inexplicable on evolutional principles, should emerge, then first would it be time to talk of a "soul." Only in this *a posteriori* (and not in any *a priori*) way, could science come in contact with this "entity," which otherwise remains an exclusively ultimate-philosophic creation, amenable solely to the canons of evidence, whatever they be, of Ultimate Philosophy.

forerunners in the pre-human, what the biological necessity[1] in this case and the neurological condition meeting it, and, lastly, does introspection throw light? The oil immersion of psychology, introspection, yields valuable hints (chiefly suggestive, confirmatory, cautionary), but is liable to all the fallibilities of interpretation inseparable from immersion work; whereas the low-power lens of the sciences (particularly comparative neurology and neuropathology), though less delicate, is interpretatively more certain. This psychology utilizes introspection within its radius of validity, and holds that had psychologists reached a consensus as to what does and what does not constitute matter of introspection, much argument about need for an Ego to appreciate likeness and difference, could have been spared.

Like all sciences, biological psychology conceives its subject-matter as exact, and subsumming under Penultimate Philosophy, it presupposes that philosophy, as specific characters presuppose the generic. For that philosophy, *why* is an illegitimate (being an ultimate) question; its *why* always means: How comes it that? This psychology, then, no more knows why the human is conscious, than the physicist why bodies "fall" together when not propped apart; as he accepts fall as matter of fact, this psychology accepts consciousness as datum of introspection. True, consciousness is a mystery, but no peculiar mystery; only one outcrop the more of the All-Mystery. We do not know what it is, but no more do we know what anything is. But answers essential for a penultimate philosophy of consciousness (characteristics and apparent nature, relations to other things, *modus operandi*), this psychology tries to give. The consciousness-series appears to be the psychic-inseparables[2] of a kinetic series finding nearest affinity in the heat series; therefore, inferentially referable to the infra-red (near Blondlot's N waves?). Like all energy, consciousness is *immanent*. Its action is neither dominant nor epiphenomenal, but reactive-dynamic and resistance-reducing. Though its omnipresence, marvels, and all-importance, form

[1] *Biological necessity:* That which *must* "somehow" come about if an organism is to survive; a condition *sine qua non* for survival. Showing biological necessity in any given case is entirely different from showing how (neurological condition) that necessity comes to be met.

[2] *Psychic-inseparable:* The "subjective aspect" ("inner face," etc.) corresponding to the various members of the kinetic series which arises through latentization of nerve energy; that which is experienced as consciousness.

Dynamic-inseparable: The kinetic species of which consciousness is the psychic expression.

Consciousness- (sensation-, thought-, feeling-) *di:* consciousness, dynamic-inseparable aspect of.

proud capstones in pretentious intoxication-philosophies, available evidence tends to show that for a penultimate philosophy of the cosmos, it is far less important; for, it is less than coextensive with the cosmos, with "life," with nervous energy even, and seems a derivative from the last under certain special conditions. Its office[1] accords with this. In evolution and individual alike, it appears not for itself, but as part of the plasticity-syndrome (plasticity-complex). The survival value of this is two-fold as anticipator of environmental happenings, and as enabler of multiple activity of one structure (through multiplicity of plastic operating mechanisms). Query whether the syndrome might have equally evolved without the conscious ingredient, is idle; *de facto*, it evolved with it (and, to judge from its *modus operandi*, as a *necessary*-incidental constituent). In the syndrome, the part inseparable from consciousness, is "profiting by experience," but a penultimate philosophy of consciousness will *not* say that such profiting is the outcome of the features in the psychic situation to which greatest significance seemingly attaches upon introspection, namely, the fore-imaging (fore-"knowing") "power" of mind. Rather will it say: The foundation assumption of scientific psychology being that every supra-liminal intensity of dynamic-inseparable has its attendant fourth-dimension absorption-spectrum (for inapt metaphor) in, and as, consciousness of a vividness in rigid correspondence with the amplitude, of a quality in equally rigid correspondence with the length, of the dynamic-inseparable wave; then, *given an awareness-consciousness at all*, along with organization of nervous connections *sine qua non* for existence (through rendering possible profiting by experience), there must have proceeded a mosaic-ing of the discrete psychic-inseparable mirror-facets into the environment-parallel awareness-patterns which make up mind. Profiting and fore-imaging are, thus, parallel concomitants (co-expressions, co-functions) of nervous organization. The psychological problem is tracing the evolution of mind. But *the* "mystery" (ultimate-philosophic question) is not that of mind but of consciousness; not why mind-mirror now images environment so perfectly, but why energy happens to have an awareness feature linked with it. But, *for psychology*, it suffices that this is datum of introspection.

Biological psychology conforms to Penultimate Logic, which, like Penultimate Philosophy, sums to acceptance of *de facto*. This logic propounds but one riddle: Is there evidence (demonstration, probability of all grades down to possibility, room

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[1] *Biological Office*: The net effect, rôle; what teleologists meant by "purpose."

for legitimate hypothesis) showing whether, *as matter of fact* this condition does or does not obtain? Before experience, nothing; from observation, facts; from facts, inductions, deductions, hypotheses, the last to be verified by observation, or where impossible, to be tested with regard to agreement (or at least non-conflict) with established fact or generalization,—that is Penultimate Logic (simple straightforward logic of science). In this logic, it matters not that the condition be unimaginable ("unthinkable"); that is beside the penultimate question. Everything is alike unimaginable; determine facts *as facts*,—then *credo, nec impossibile*.

This essay aims at a theory as to nature and mode of action of consciousness which shall be all-embracing, conflict with no established fact or generalization, accord with the law of the conservation of energy, conform to accepted modes of scientific reasoning, and conflict with no datum of introspection. It shows how an anomaly of carbon hitherto ignored, explains (1) why carbon compounds alone admit of that complication of molecular structure characteristic of "organic" compounds; (2) why the period antecedent to the emergence of "life" may well have been one of progressive molecular complication; (3) how, with transit of impinging energies, only carbon compounds undergo modification in such manner that resistance is less for subsequent transits than for the initial transit, which modification being the basis of the most fundamental *sine qua non* for existence, profiting by experience, in carbon compounds alone can the phenomena summing to "life" come to be manifested; and (4) how (*a*) in the intensification-stage of this process in nerve tissue, involving emergence of consciousness (as a critical phenomenon when the relation between nerve energy and resistance comes to exceed a certain "limen" value), the reactive resistance-reducing effect of consciousness eventuates in unlimited profiting by the individual experience, and (*b*) subliminal recession of the emerged consciousness takes place in connection with expenditure in intra-molecular work. With these conditions as bases, and utilizing electrical principles, attempt is made to explain the principal varieties of nervous action; conduction in the infra-liminal ad-cortical and ab-cortical areas, and especially in the labyrinthine mazes of the cortical-psychic zone. Above all else, solution of the cortical-psychic problem is sought, that the equation of the conservation of energy within the living body may be knit up. Three "mental elements" are recognized, *and defined*; and analysis forces admission of distinct molecule-types to account for non-lapse of sensations and their dependency of function upon intensity of stimulus, for lapse of thought, and for non-lapse and dependency of function upon intoxication conditions, of feelings.

And it is found that no need exists by reason of any psychic fact, for impugning the universality of the law of the conservation of energy; rather, there is every warrant for holding that equally with all physical facts, all psychic facts subsum under that law.

Despite dogmatic tone, inevitable considering the brevity, this essay is *all theory*,—just like Professor So-So's big good volumes, but here theory is labeled. No preliminary attempt like this can be the last scientific word on consciousness; ample indeed would it be should this attempt prove the first such word. But one thing is certain; if it be not such first word, that word yet remains to be spoken. *The* desideratum for psychology is *some* hypothesis which shall bring mental phenomena into a connected whole; for no science can progress in advance of formulation of its basal theory.

Had conditions admitted, blemishes inevitable with defective vision would have been avoided. But the facts appealed to are well known, and the only especially original feature is the view point; orientation of data round the electrical anomaly of carbon and dynamic-neurological conception. The rest is culture-material whose source is often forgotten, and for which I cheerfully admit indebtedness to nearly every one.

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CHAPTER I

Biodynamics and Biogenesis

Anomalous electrical behavior of carbon; explaining why carbon alone forms basis of "organic" and *a fortiori*, of "life" compounds, its compounds alone being susceptible of modification under impact of energies, in such manner that reaction to subsequent such impacts is less difficult than reaction to the initial impact ("profiting by experience," origin of "genetic" series). Presumable difference in degree of anomaly among the various carbon compounds, affording a basis for the line of increasing molecular complication leading, in absence of microorganisms to effect decomposition, to compounds capable of manifesting *n*—*a* of the phenomena summing to "life," to which might come to be added the remaining *a* properties. Conceivable modes of development of reproduction and motion. Lines along which, *de facto*, variations came to accumulate; the plant line and the animal line, and the dynamic characteristics of each.

Carbon exhibits an anomaly all-important for explanation of "life" phenomena, its electrical resistance *decreasing*, whereas with every other conductor[1] resistance *increases* upon heating. And it carries this anomaly into at least some of its compounds (wax, paraffin, gutta percha, vulcanized rubber,

[1] Except antimony, an exception to be supplied *wherever* "carbon compounds alone" (or equivalent phrase) appears.

ebonite). Decrease in resistance upon heating must likewise take place with gases, they conducting better when rarefied; such decrease also occurs with sulphur (a "non-conductor"), and is a feature of electrolytic conduction.

Comparing, now, behavior toward the electric current, of metallic salts, *e. g.*, zinc chloride, with that of carbon compounds, supposing elements to carry into their compounds all their properties (however masked these may be), the heat emerging upon passage of the current will entail increased resistance in the zinc, but decreased resistance in the chlorine. This assumption by the component atoms, of opposite conductional phases, accords with (and probably explains) their observed readiness to separate, in electrolysis. For, if conduction mean capacity for harmonic vibration, and if molecular integrity be contingent upon the same capacity, then such assumption will introduce vibrational discord incompatible with the continued existence of the molecule.

The case must be the same with carbon compounds such as carbonates, into which a metal enters; accordingly such molecules remain quite simple and are not classed as "organic." Quite different would seem the case of compounds of carbon with elements in which resistance decreases upon heating; here, the heat effect of the current tends to bring all component atoms alike (though probably not in equal degree) into that harmony of vibration implied in the better conduction. Consequently, in such molecules, conduction will come increasingly to supplant decomposition, any resistance tending to become eliminated through reactive effect of the entailed temperature-increase. We thus reach molecules less prone to shattering under action of impinging energies; the possibility of indefinite molecular complication and of the colloidal state.

But some carbon compounds must possess capacity for reacting to energy-impacts in the way of conduction as against decomposition, in higher degree than others; to maintain the contrary, is to assert that the other atoms contribute nothing towards the properties of the molecule. And when such a molecule enters into higher combinations, the resulting super-molecules will be, as compared with the parent-molecules, either equally capable of conduction and decomposition, less capable of conduction but more so of decomposition, or more capable of conduction and less so of decomposition.

Neither of the first two exhibits nearer approach to the vital condition, but the third possibility does; and if amid numerous compounds of this type, one emerged in which in the presence of certain energy-conditions, an atom-group should become split off, with which in the presence of other energy-conditions, the molecule-remainder could recombine, we should

already have a crude metabolism. And just such compounds are the enzymes.

These ferments, indeed, possess metabolism (capability of back-and-forth play of take-up and let-go), resembling that of "living" compounds in (1) being quantitatively unlimited as long as (2) undue accumulation of products of their action is prevented, such accumulation inhibiting further activity, with ferment and living compound alike. And the only difference of moment between the two classes of compounds is that the living possesses capacity for "growth." For, of characters considered distinctive of "life," reproduction is a derivative of growth, and motion seems a physical-chemical reaction to environment.[1]

But growth is merely predominance of intake over output; instead of *immediately* after assimilating a molecule, disgorging in decomposition-products its exact equivalent, the life compound disgorges products equivalent to only a portion of the intake, the remainder being held combined, for a longer time.

Motivated by the growth force, the life compounds tend to grow till some counter-force comes into play; but mass increasing as the cube, surface (digestive, respiratory) only as the square, growth must have *some* limit. The sphere representing minimal surface for given mass, larger masses can accumulate in elongate-flattened form. But this form involves frangibility leading to fracture under impact of incident forces (wave action). The fragments, however, can grow to the original size, and reproducing the original frangibility, will repeat the fracture.

Crude as such reproduction would be, its importance cannot be overestimated; for the elongate-flattened forms will thenceforth increase by geometrical progression, while non-fracturing spherical forms will increase only by accession of newly evolved molecules. But causes of decrease (food scarcity) will operate about equally, elongate-flattened forms having even here the advantage incident to greater surface. Thus, even before the advent of carnivorous metabolism, there will be a steady trend toward numerical predominance of elongate-flattened forms. But when over-increase comes to entail extinction, variations in the direction of optimum orientation of the plane of division will accumulate through Preponderant Survival;[2] and when nuclear substance appears, that optimum will lie in such orien-

[1] *Cf.* Bütschli, *Ueber Schäume*; abstracted by Dolbear, *Woods Hole Lectures* for 1894.

[2] Preferred to "natural selection," as expressing nothing but the facts, and avoiding implication of selecting agency; also, as available equally in the form, Preponderant Extinction, which emphasizes the real

tation of the division-plane, as entails equal substance in each daughter-cell.

Paradoxically, defeat of "spontaneous generation," furnishes an argument in its favor. To-day it might be impossible for "life" to develop *de novo*, unless under artificial conditions; but why? Because the necessary forerunners, the long series of gradually complicating molecules, would be destroyed by microorganisms. So accustomed are we to this result of their activity, that we speak of "easily decomposable substances," forgetful that there are no more stable compounds than these same, in the absence of an efficient cause for their decomposition; and the myriad laboratory and commercial sterilizations prove that the only such cause is activity of microorganisms, no combinations of physical energies, sufficing. But such compounds never breaking down "of themselves," the period antecedent to the emergence of "life," may well have been one of progressive complication of carbon molecules; for, decomposing agencies absent, such molecules may have remained unaltered indefinitely, till change in environmental conditions led to further complication, however delayed the advent of that change may have been.

Among these earliest forms, variations toward increased rapidity of growth and a prodigious prolificness,[1] would accumulate, provided with such variations there did not happen to be associated others overbalancingly contra-adaptive ("unfavorable"). But variations in the direction of rapid growth and reproduction do not exclude other variations, simultaneous or successive. Consequently, individuals predominating numerically in virtue of prodigious prolificness may perpetuate variations arising haphazard (*de facto*, from unknown causes), till, sometime, those variations either (1) disappear, or (2) become functional through (*a*) increase in size, or (*b*) emergence of some new haphazard variation which with the previous such variation, constitutes a combination having survival value. But, become functional, the previous variation, *viewed retrospectively*, is termed an "incipient structure," a name convenient enough if it do not imply difference between the variations which emerge only to disappear, and those which emerge later to increase. Both are alike variations, and subsequent disappearance of the one set, and subsequent elaboration of the

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factor, "selection" of some organisms, really meaning their presence as living representatives, by reason of extinction of the remainder. But having most to do with living forms, the affirmative phrasing will be most frequently used.

[1] Here, prolificness refers only to number produced (fecundity); not to proportion surviving to maturity.

other, are equally incidents of prevalent environmental conditions.

In the beginning, successive integrations of structure must have been limited to accumulation of haphazard adaptive variations; especially would haphazard *combinations* of adaptive variations accumulate. At first,[1] such combinations must, on the doctrine of chances, have been exceedingly rare; and consequently, only among innumerable progeny of prodigiously prolific individuals, would they occur in sufficient number. Their elements need not, however, have appeared simultaneously, if one element were sustained by the carrying power of prodigious prolificness, till such time as the other (or others) appeared.

Organisms, then, first predominating numerically by reason of prodigious prolificness, might come later to survive preponderantly (be "selected") in virtue of other characters originally acquired incidentally. But sprung from prodigiously prolific forms, such organisms would, nothing opposing, remain prodigiously prolific,—whence, the condition exhibited by protozoa and protophyta: adaptive reactions and structures in combination with enormous prolificness, the last considered adaptation to conditions involving enormous extinction, though it is perhaps not only (and possibly not mainly) referable thereto, but may be a relic of the original sustaining factor.

Probably the characters considered distinctive of "life" did not emerge all at once; the primordial organisms most likely possessing metabolism only, eventuating in increase of mass ("growth") and reproduction. But the advent of carnivorous metabolism may have been long delayed. Whether that advent ante-dated, accompanied or post-dated that of motion, is indeterminate and relatively unimportant; combination of these activities opens up the illimitable vista of the struggle for existence with its incidental outcome, preferential survival of particular individuals.

Motility may have emerged through accumulation of haphazard variations. If among innumerable offspring of prodigiously prolific individuals, some varied in the direction of greater tremulousness of the protoplasmic jelly, and if among offspring of these individuals such greater tremulousness persisted indefinitely, till later (one or a million years) in some tremulous individuals, variations arose in the direction of correlation of tremulousness into reaction to impact of energies ("stimuli"), such oriented tremulousness would be "motility,"

[1] At first; progressive preponderant survival of individuals in which adaptive combinations have, *de facto*, arisen eventuates in a race prone to emergence of such combinations.

the problem in the origin of which seems not so much that of change of form, as association of such change into reaction to impinging energies. But capacity for response to environmental happening being the only element in external reactions of survival value, and accuracy of response the final measure of that value, variations in the direction of greater accuracy would accumulate; variations, that is, in the direction of integration of structure, in proportion as they conduce to such accuracy, to improved sensori-motor connections.

Accumulation of haphazard variations thus eventuates in integration of structure to a point at which the organism becomes able to react adaptively to recurrent environmental happening; when, perfect adjustment reached, modification ceases. But subsequent change in environment will entail consecutive change in habitant organisms, via the same process,—accumulation of haphazard variations adaptive under and to the new conditions. As with linking of variations into combinations, so with linking of new variations on to those combinations, the process must have been exceedingly slow, dependent as it was upon emergence of variations, and extinction of the vast host not possessing them; and only among offspring of the prodigiously profilic would sufficiency of variations occur.

But with every winnowing out of individuals unpossessed of survival-requisite variations, the "successful" remainder comes to be more and more exclusively made up of individuals in which environment-responsive variations have, *de facto*, emerged; therefore, of individuals more uniform in type, as being out of many the particular ones which happened to coincide in adaptation to common environment. An incidental outcome of successive winnowings must then be an ever more pronounced and apparent definiteness of evolutionary trend, series ever narrowing into closer conformity with environmental conditions, necessarily eventuating in an environment-parallel race. And the evolution of that race will thenceforth exhibit a definiteness of trend in keeping with whatever definiteness of trend happens to obtain in environmental conditions. Whence, no presumption of a "plan of evolution" (equally none as to design in the universe) is inferable from this definiteness (secondary and derivative) of biological trend, however successfully or unsuccessfully it may be inferred from the parental definiteness of environmental trend.

Prodigious prolificness involving frequent re-divisions of the protoplasm with corresponding brevity of the nutritive stage, precludes growth to larger mass. But when the original numerical predominance of prodigious prolificness gives place to preponderant survival in virtue of integration of haphazard adaptive variations into combinations (capture and digestion of

prey), further accumulations of such variations, in the direction of increased surface (digestive), may eventuate in larger mass with its ulterior possibilities in the way of differentiation of structures. More abstractly, the carrying power may come to pass over from the prodigious prolificness to the adaptive combinations, obviating previous hindrance to growth to larger mass.

From the prodigiously prolific mass whence descended our protozoa and protophyta, variations accumulated, *de facto*, along two main lines, eventuating in plants and animals. The momenta (organismal dynamic factors) determining trend of evolution along the plant line, seem to have been those previously operative,—successive emergence of haphazard adaptive combinations sustained by a prodigious prolificness. For the momenta distinctive for plant evolution are: Enormous expansion of surface in association with fixed habitus and hard protective structures, and relatively prodigious prolificness, eventuating in larger mass; with, probably, proneness to emergence of adaptive combinations, bespeaking descent from individuals in which such combinations had, *de facto*, been wont to occur. At least greater frequency of "sports" in plants, and the fact that most if not all asserted cases of genesis of new species come from botanists, speak for this view.

The momenta determining trend of evolution along the animal line are: Moderate expansion of surface, such sufficing in virtue of association with active habitus, the latter sustained by emergence of structures admitting of alternate and multiple uses of the same organ and of "profiting by experience," eventuating in larger mass; and, following the advent of the psychic factor, progressive decrease in prolificness in connection with an ever-increasing capacity for self-protection, securing of the opposite sex, and care of offspring, summing to a continually rising multiplication rate. Otherwise put, the sequelæ of the emergence of the psychic factor were a steadily increasing tendency to readjustment of survival values, toward a shifting over from survival of the more prolific to that of the more intelligent, and an incidental outcome, integration of "mind," has been the steady trend toward individualization.

CHAPTER II

NEURODYNAMICS

Neuromolecular readjustments consecutive upon environmental happenings, whereby optimum motor response to such happenings comes about; optimum as regards kind, intensity, and rapidity of response. Interpolated inquiry whether nerve energy is electro-chemical. Parallelism between reactive effect of heat in the electric circuit upon the conductor, and that of consciousness-*di* upon nerve substance; indicating the *modus operandi* of consciousness as neither dominant

nor epiphenomenal but reactive-dynamic, and further indicating the nature of consciousness, its mode of genesis out of nerve energy, and its subliminal recession as incidental to its expenditure in intra-molecular work,—all in harmony with the conservation of energy.

In way of external nervous reactions, the one *sine qua non* for survival is accurately adjusted motor response to environmental happenings. Loosely termed "profiting by experience," such adjusted response is the outcome of several neurological processes, interblended and separable only in analysis. But clear understanding of reactions ("actions") of animals, can be attained only through preliminary analytical separation and ultimate synthesis, of these processes. Motor response can become adjusted to happenings as regards kind, intensity, and rapidity, of response.

Adjustment as regards kind of response. The following are involved:

Depositional-registrative processes:

1. Registration of environmental happenings. *Imprintation.*

The real type, of which 1 is an abstraction, is, however,

1 *a*. Registration of imprints, *chained in series*, consequent upon environmental happenings chained in series. *Serialization.*

Resuscitative-interpretative processes:

2. Appreciation of environmental happenings *as having been experienced before. Recognition.*

2 *a*. Appreciation of environmental happenings as having been experienced before, not as individual happenings, but *as members of a chained series. Diagnostic recognition.*

All these processes find explanation on ground of that property (variation in resistance with temperature) of conductors, in virtue of which mere passage through them of the electric current entails change in the *future relations* of current and conductor. And with a current dividing itself among several carbon conductors, the ultimate distribution will be different from the initial distribution, a disproportionate amount of current coming to pass via those conductors which, originally offering greatest resistance, suffered most heating. True, there is no evidence whether with carbon itself, one passage of the current would entail lessened resistance for subsequent passages (initial temperatures equal). But that with certain carbon compounds, one passage might entail lessened resistance for subsequent passages, follows from Spencer's conception of the colloid molecule as one in which complexity of play and cross-play of the intra-molecular forces is so great as to induce molecular sluggishness (inertia, viscosity, so to speak), a con-

dition in which change takes place less readily and goes less far than with crystalline molecules under like conditions of impact; but likewise, as with a molecule in which, change once occurred, return to the original condition is correspondingly difficult and imperfect, the molecule retaining some imprint of its experience. Whence, here we reach the origin of "genetic" (irreversible) series, the philosophic import of which can, therefore, be no greater than that attaching to the underlying physico-chemical change in the substrate. But among the carbon molecules manifesting such *plasticity*, those exhibiting "life," are the ones in which it is manifested in highest degree, and among these, the molecules forming the nervous substrate are the ones in which it is manifested in highest degree of all.

Of the above catalogued processes, the plasticity of the carbon molecule explains Imprintation and Serialization,—the former, as transit of energies through such molecules modifies them for subsequent such transits; the latter, as mechanisms once associated in action are thenceforth interconnected by an imprinted line of lessened resistance.

Respecting Recognition, recalling Spencer's conception and such phenomena as isomerism and polymerism, we may picture flow of energy through the nervous system thus: Impinging energies in undergoing absorption, entail unequilibration in the peripheral sense organ, eventuating in a nerve "impulse." Concerning the nature of this, if one will have any working hypothesis (any principle of coherence among his ideas, that is), but one is possible in the present state of science,—nerve impulses are *some* form of undulatory energy. Among known forms of energy, their affinities being closest with electricity, electrical phraseology is admissible, pending emergence of inconsistency or *reductio ad absurdum*. Depicting the changes ensuing in an undifferentiated neurone[1] upon the advent of nerve-impulse waves, potential accumulating to the irruption point, discharge along the neurone occurs, under resistance, entailing emergence of heat which reacts, bringing about such modification of orbit form (vibration system) of the unit-molecules of the neurone as corresponds to diminished resistance (say the ellipsoidal). On account of the unwieldiness, sluggishness and viscosity of the molecules, reorientation of orbit form is at first slow and imperfect, and on cessation of stimu-

[1]Nerve cell plus "fibre;" unit of nervous architecture. The neurones of the intervertebral ganglia extend, one process down to feet, the other up to the medulla. The neurone theory is here used for convenience of presentation; present speculations are not dependent upon it for their validity, much of this line of reasoning dating back to the days of the vaguer "tracts."

lation, the orbit form swings back toward the original figure (say spherical), return being, however, incomplete. But with each recurrence of similar stimulation, the orbit form becomes warped further in the same direction, and every back-swing is less.

While subsequent conduction thus becomes more easy for the same stimulus, it becomes less easy for stimuli of other type; for orbit forms modified in one direction will be less susceptible of modification in other directions than will original unmodified orbit forms,—the experience-imprinted molecule becomes specialized.

Though sometimes, Recognition is not necessarily (or even generally) accompanied by consciousness; most movements being automatically adjusted, *e. g.*, proportioning of pulls of the various leg muscles to unevennesses of the ground. But such proportioning presupposes Recognition as an indispensable interpolation in the sensori-motor progress. How explain such automatic proportioning? On sensory stimulation, nerve-energy waves surge up to the terminal arborizations of the first tier of afferent neurones where they heap up a certain "gradient."[1] If, now, nerve energy be some form of wave motion, different varieties of that energy (those initiated by light waves of different lengths and eventually arousing different sensations) may be assumed varieties of such motion, with different lengths or different vibration rates. And the only explanation accordant with accepted scientific theory, of the better conduction along previously used routes, is that it is a phenomenon of harmonic-vibration type, and the non-conduction along routes otherwise specialized, a phenomenon of interference.[2]

If, then, among the innumerable molecule-chains constituting the second tier of neurones, one has through repeated imprintation under the influence of identical (perhaps also of nearly identical) stimulus waves, become specialized for transmitting them, they will outflow along that neurone in preference to others, not necessarily that they might not eventually have heaped a gradient steep enough to admit of their irrupting into those others, but because *at a time antecedent* to that at which such gradient could be attained, a gradient was

[1] "Gradient" for consistence of phraseology; "unequilibration," less specialized, would answer as well.

[2] I have seen in successful operation a device for calling at will any particular subscriber on a "general circuit," consisting of armature-tipped metallic reeds, one at each subscriber's, and at the telephone exchange duplicate reeds, each attuned to unison with that of one subscriber. When the electric current was switched through any reed, only the bell of the subscriber with the correspondingly attuned reed, rang, interfering pulsations preventing transmission of current through the other reeds.

reached at which the specialized neurone was "permeable." Specialized neurones are, thus, the *antecedently permeable*, meaning thereby that to every increment of rise of potential, there corresponds a vast number of approximately permeable neurone-conductors, and the one among the many possibles which a particular wave enters, is that permeable for it at lowest (first-reached) potential. But, discharge having occurred along the lowest-potential route, such further rise of potential as might have admitted of irruption into less permeable routes, is precluded. Antecedent permeability is the key to conduction-conditions everywhere throughout the nervous system, and it means that with every flow of nerve energy, certain preferential routes will be followed. But this is Recognition, Diagnostic Recognition, the essence of which is *discriminative* preferential conduction, discriminative with respect to the shunting of the initial member of a stimulus-series into the route traversed by its identical (or closely similar) predecessors.

If the stimulus be an unhabitual one, no specialized neurone existing, potential will rise to a gradient at which irruption can take place into an unspecialized plastic neurone, the resistance of which, though greater than that of a neurone specialized for a particular stimulus, for that stimulus, is less than that of a neurone so specialized for a stimulus other than its own.

Adjustment as regards rapidity of response. But the only direct value of the nerve apparatus in the struggle for existence being its ability to fore-parallel environmental happenings, admitting of protective reaction not to the happening of the moment, but to its coming sequels in advance of their advent, 2 and *2a* (an analytical abstraction *en route* to *2b*) have no survival value except as they enter in *2b*.

Accelerative processes:

2b. Appreciation of environmental happenings as having been experienced before, not as individual happenings, but as members of a chained series, *the remaining members of which are yet to come—Diagnostic Recognitive Fore-triangulation.*[1]

But the value of this process would be rather limited, did not an identical one occur on the motor side.

3. Ability to execute more rapidly movements performed before—*Agilization.*[2]

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[1]Hereafter termed *Anticipations; Expectations* of Huxley (*Life of Hume*).

[2]The essential features here are: Increase in rapidity and in efficiency (co-ordination plus power), which we observe increase *pari passu*. "Greater ease upon repetition" is the same thing in psychical terms.

But happenings varying widely in exigency, only organisms can survive which have become able to fore-parallel environment. Neurologically, increase of rapidity upon repetition is the outcome and expression of a progressively decreasing time-consumption upon subsequent, as compared with that upon the initial passage of the nerve-energy current, in reorienting the viscous, sluggish, unwieldy molecules, which undergo reorientation *seriatim* through, and only in proportion to, heaping in the first molecule, of potential to a point at which latentization takes place, whereupon a certain amount of reorientation of that molecule ensues, admitting of passing on of the current, in quantity, to the next similar molecule, in which the same process then goes on.[1] The time-consuming element in the process is the latentizing, which necessarily takes place constantly at expense of potential.

This explanation of increase in rapidity is supported by parallellism between progressiveness of that increase, and that of the attendant psychic phenomena ("greater ease" and diminished vividness of consciousness), to which latter the foregoing explanation is particularly apt.

But imprintation makes for rapidity, not only by impressing the reorientation-habit upon successive molecules of one neurone, but more by perfecting inter-neuronal connections ("associations"), and most of all by entailing the turn-table reorientability of the plasticity zone (which ensures concentration of conduction indispensable for rapidity), and replacement of conduction via conscious, by conduction via reflex tracts. But all these are incidental and inevitable step-by-step elaborations of the one process of molecular reorientation-imprintation.

But when rapidity of conduction along the happening-indented, environment-paralleling imprint-chains comes to exceed the rate at which happenings succeed one the other in the environment, on advent of stimulus annunciative of happening *a* of the happening-series *a-n*, the entrant waves sweeping along the much-imprinted nerve trellis-work ahead of the drama-development, before the advent of the stimulus annunciative of *b*, the organism is already fore-reacting to the impending threat inherent in *k* ("instinctive warding off"). But this is Diagnostic recognitive fore-triangulation and Agilization. When accompanied by consciousness (not at all necessary), the process is heralded as "fore-knowledge" (fore-imaging), but conscious or unconscious it equally consists, in

[1] O. J. Lodge (*Internat. Monthly*, 1900 (May), I, p. 495) takes essentially the same view (as far as this point goes), thinking conducting substance comparable to a series of condensers, charged *seriatim* to full capacity; and this is, likewise, his explanation of rate of transmission. His "capacity effect" is my "reorientation."

essentials, in resuscitation of fossil experiences imbedded in the imprint-chains, and eventuates in repetition of the motor response which, *de facto*, sufficed in former emergencies.

INTERPOLATION: IS "NERVE"-ENERGY ELECTRICAL?

Eccentric, bizarre theses apart,[1] nerve energy is a kinetic species, either distinct from all, or identical with some, species operative outside the body. So long as it is conceived as a species of the energy genus, interchangeable with other species at exact (as yet undetermined) equivalent, the difference between the alternatives is not fundamental; still they have very unequal merit. If one elect the first and, without the necessity being proven, begin multiplying energy-species, where will he stop? If "nerve" energy shall be a distinct species:

Hypothesis. Difference in microscopic structure and in function constitutes a valid criterion of distinctness of associated energies, or—

Hypothesis. Such difference does not constitute such a criterion.

On the first, nerve, muscle, gland (etc.) energies are distinct kinetic species. But if to every large difference in structure and in function corresponds difference of species, for minor (but well-marked) differences must be admitted varietal differences; four varieties for the connective-tissue group, three for the muscle group, two for the nervous system,—more for glands, mucous membranes, etc.

The second hypothesis avoids this confusion, leaving clear way for assuming the foregoing factitious species and varieties to represent extra-corporeal energies, here peculiar in *facies* by reason of peculiarity of substrate; an assumption in accordance with all known facts. For, intra-corporeal heat, electricity, light, differ in no wise from extra-corporeal; and every chemical advance makes increasingly manifest the identity of intra-corporeal with extra-corporeal chemism. And the substrate which, in homologous muscles in allied genera and other muscles in the same fish, generates "contractile" energy, is in certain muscles of some fishes modified to an electricity-generating substrate.

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[1] As that nerve energy is a Something-not-energy. Our meek patience with vagaries where consciousness is concerned does not extend to such theses *in re* nerve energy; and fondly we hope the "philosopher" of the future will be limited to the alternative of admitting consciousness to be, or of denying that nerve energy is, a kinetic species. Whoso wills to maintain the latter must explain:

1. Potential energy of food excluded, *ex hypothesi*, from serving as source of this Something-not-energy—(*a*) whence is this Something derived in embryo, from spermatozoon, ovum, or both? (*b*) in growth from embryo to adult, what is its source of increase? (*c*) from what source is it kept up from day to day?

2. (*a*) On the assimilative side, where do ordinary forms of energy cease to give place to this Something? (*b*) not interchangeable with, what is the relation of it to, ordinary forms of energy, and their relation to it; and how is the equation of the conservation of energy within the living body, all at loose ends, to be knit up? (*c*) on the dissimilative side, where does this Something cease to be, and ordinary energies again come into play, and what is the destiny of this Something after it has done its work?

3. Are nerve, muscle, gland (etc.) energy merely different modes of manifestation of one Something-not-energy, or distinct Somethings-not-energy? If the former, then muscle energy being in some cases demonstrably electro-chemical, the case falls. If the latter, heavy *onus probandi* lies on the asserter, to show wherein the case for nerve energy differs from that for muscle energy; and if histological differentiation shall serve as criterion of distinctness of Somethings-not-energy, to show where multiplication of such Somethings is to stop (striated-muscle Something, smooth-muscle ditto, heart-muscle ditto, and what not).

"Trivial!" I think it is myself, but for outspoken declaration that consciousness is Something-not-energy, yet can control energy, see Minot (*Pop. Sci. Mo.*, 1902, lxi, 289-303.)

Eccentric bizarre theses apart[1], the hypothesis on which one can correlate ("explain") most facts with fewest difficulties, is that nerve energy is electro-chemical-electric,—meaning consisting of waves of electric (or near-electric) type, set going in the sense organs by stimuli, which waves excite in nerves (perhaps mostly in the cells) chemical changes generating tributary reinforcing similar waves.

To the view that nerve energy *may be* electricity adequate objection has never been made. Difference in speed has been appealed to; *meaning* difference between speed of nerve energy in nerve, and of electricity in wire. Speed of electricity in nerve has never been measured (how could it be?); what has been measured is speed in nerve of an electrically generated nerve impulse. But transit of electricity through a series of sluggish, unwieldy, viscous molecules which have to be reoriented (via the heat effect) *seriatim* as it goes along, presents a condition not at all comparable with transit along a wire.

Also, nerve energy is said, unlike electricity, never to cross a gap (between apposed proximal and distal ends of a severed nerve). Even if undisputed, this would not necessarily invalidate the theory of identity; too many elements of uncertainty existing (impossibility of excluding diffusion of current around cut ends, improbability of apposition of original neurone segments when the cut ends are brought together). Further, the only way that, in view of the phenomena of electric currents in nerve, the natural inference of the identity of nerve energy with electricity has been staved off, is by asserting (very probably correctly) that the currents are "currents of injury," the outcome of changes taking place over the exposed cut ends of the nerve. But this explanation cannot be worked double; if the cut ends are sufficiently altered to generate "currents of injury," they may well be so altered as not to transmit nerve energy. Finally, cases of rapid restoration of function have been accumulating in recent surgical literature to an extent to render it probable that the whole question of non-transmission across a gap may yet come up for re-hearing with the ultimate verdict in doubt.

If nerve energy be electrical, everything clears up,—its source is the chemical energy of the food, it enters the equation of the conservation of energy, it can induce muscular contraction as electricity induces such contraction. Moreover, its latentized form (the psychic aspect of which we know in and as consciousness) reacts upon nerve molecules as the similarly generated by-product of electricity, heat, reacts upon carbon molecules. Consciousness here finds no mere analogy, but a true homology of relations, and its first parallelisms of relations, former comparisons being mere fanciful illustrations, ikons to assist the imagination of the vulgar to "understand" unintelligible things. The homology may be thus shown:

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[1]As that nerve, muscle, gland energy are merely different manifestations of "vital force."

Hypothesis. "Vital force" is Something-not-energy.

Hypothesis. "Vital force" is a kinetic species.

The first requires answer to all questions in note, p. 77; and, in particular, explanation how in electric fishes muscle Something-not-energy can be replaced by electricity.

Though the second seems expressly designed unerringly to miss every advantage and infallibly to catch all difficulties in the situation, though exceedingly improbable (almost demonstrably false); so long as things fall within the equation of the conservation of energy, we, become old and patient, might make shift to live with it, cursing it roundly for the clumsiest contrivance it was ever our ill-starred fortune to meet.

Electricity	Nerve Energy
Working against resistance, heat emerges.	Working against resistance, consciousness emerges.
Heat diminishes resistance in carbon molecules.	Consciousness diminishes resistance in nerve (carbon) molecules.
In such molecules, the heat of the electric circuit tends to eliminate itself.	In such molecules, consciousness tends to eliminate itself.
Better conduction is the result.	Better conduction ("profiting by experience") is the result.

Adjustment as regards intensity of response. But how do the resistances become so nicely graded as to account for all the minutiæ of adjustment observable? Environmental happenings differing widely in violence, only organisms can survive which come to be able to react to different such happenings with very different intensities of motor response; horses must not waste on escape from flies energy needed for flight from the wolves, salmon bound up-stream on sexual mission must expend every kinetic unit to surmount the cascade. That is, graded happenings must be met by graded response. But intensity of response is determinate as (variations in intoxication-condition apart) the result of the kinetic outpour of the plasticity zone. That outpour is, however, determinate, as the resultant, in magnitude and direction, of the forces undergoing fusion (superfusion, etc.). But those forces are determinate, as resultants of amount of nerve energy entering, and of its lines of transmission within the plasticity zone; these factors involving (as dependent function) liberation of a determinate amount of energy by the cell elements of that zone. Amount of entrant energy and lines of transmission are, however, determinate; amount as a dependent function of intensity of stimulus, lines as quotients inversely proportional to the resistances. Whence, graded motor response can come about only through grading of intensity of stimulus, of resistance, or of both. But, being environmental, stimulus is for the organism a fixed quantity and ultimate factor; consequently, graded response can come about only through grading of resistance. And stimuli being graded, it must come about. For, among carbon molecules differing in initial resistance and capacity for ultimate modification, preponderant survival will occur of individuals offering the optimum, which will lie where initial resistance suffices to develop just the amount of heat which will reduce resistance to optimum conduction, measured in terms of final œcological value, motor response; just that heat which will expend itself in intra-molecular work. For, if by reason of too small, or by reason of too great, initial resistance, heat relatively insufficient emerges, conduction along that molecule-chain will be deficient, eventuating in deficient response. Contrariwise, heat relatively too great will entail unduly great conduction along that particular molecule-chain and consequent disproportionately great response of certain muscles (inco-ordination).

So far, the hypothesis demands a separate molecule-chain for every intensity of every stimulus. But biological economy (most needed in the brain with whose rapidity of enlargement the fundamental process of bone-development in cartilage has been unable to keep pace, whence cranial-vault development in membrane) suggests that each molecule-chain may transmit several (or many) intensities of stimulus; that each chain has a certain polyvalency.

CHAPTER III

PSYCHODYNAMICS

I. **Factors Determining Direction of Energy Flow:** Fundamental dynamic constitution of nervous system (stabile factor); its persistence temporarily, spatially, zoölogically. Intoxication condition of nervous system (labile factor); principal variations in reaction type. Stimulus (environmental, annunciative factor).

II. Cortical-Psychic (Conscious) Activity: No thaumaturgy, but biological resultant contains reactive as well as incoming components. Phylogenetic divisions of cortex. Bioneuropsychodynamic relation. Definition of consciousness-facies,—sensation, thought, feeling; their differences referable back to differences in underlying arousing conditions; their respective dynamic rôles. Law of lapse, of fade. Taxonomic importance of lapse.

A, Pre-Compositional: Non-lapsing facies take rise in elastic-molecule structure. Sensations, non-lapsing by reason of biological necessity for space representation; latter, outcome of interproportionment of simultaneous sensations parallel with interproportion of arousing stimuli. This requires constancy of resistance in sensation area; which constancy ensures projection forward into coming composition of forces, of stimulus waves environment-representively interproportioned. If nerve energy is electrical, heat-generation in nerve circuit is directly as energy-radiation in environment, fulfilling biological necessity for environment-representation in cortex; comparison of Fechner's with electric-heat formula suggests his constant measures resistance of sensation area, his logarithmic function judgment-coefficient from distance-relations of moving objects, and correlation of psychic-inseparable with dynamic-inseparable on basis of increment for increment. Inheritability; Baldwin-Osborne-Morgan "factor" superfluous. Consciousness penultimate-philosophically.

B, Compositional: Plasticity-zone action divisible into Route, Fusion-Composition-Reorientation-Psychogeny; repetition of previous activity except for differences the outcome of imprintations or of variations in "functional condition." The last apart, Route is strictly a dependent function of imprintation conditions; determines character of reactive forces but not their amount. Fusion: Abutment area; inequality of imprintation inevitable outcome of unequal frequency of happenings, leads to formation around abutment area, of fusion focus. Imprintation along intra-, exo-, and extra-focal routes, contrasted. Attuning of molecular orbit forms; reciprocal modifying action of wave on orbit form, of orbit form on wave. Beginning reconcentration involves fusion. Composition of forces: Only another side of fusion; amplitude of fusion wave sum of intensities of component waves. Superfusion-supercomposition. Reorientation: Incident to fusion-composition; its essence, dynamically, redistributions in kinetic field whose outcome is optimum motor response.

C, Concurrent-Compositional: Given sensation (space-orientation) and feeling (reaction-regulating) ingredients, organisms could oppose appropriate response to happenings; reaction-sequences *sine qua non* for existence could come to be motivated through adjustment of second factor, via Preponderant Survival. Feeling and thought independent variables; their respective offices; that of feeling, rendering of tracts temporarily reflex, ensuring, without fatigue, certain prompt performance of sequences too long-lasting and complicated for execution via reflex action. Alternative, that migration-sequence thought-motivated, untenable. Thought, *the* prophetic sense. Introspection and self-consciousness. Allocation of feelings, to motor area or special affluents of it. Independent of intensity of stimulus,

feelings cannot be "in the sensation;" not lapsing, cannot take place in thought tracts. Sensations presumably tributary, some to pleasantness, some to unpleasantness, tracts. Dynamic interrelation of feeling and thought tracts; probably former on resistance-plateau accessible to energy-inflow when intoxication lowers resistance; while energy can flow from feeling to thought tracts, reverse flow contingent on intoxication-lowering of resistance. Relative permeability of feeling and thought tracts, outcome of accumulation toward optimum intoxication conditions and optimum plasticity, respectively.

III. Infra-Liminal ad-Cortical and ab-Cortical Neurones: Constant but low resistance; composition cannot take place by reason of biological necessity; this molecule type probably phylogenetically the older.

I. FACTORS DETERMINING DIRECTION OF ENERGY FLOW

The factors determining large fluctuations in energy flow, are divisible into two classes,—those which go to make up the selective receiving capacity of the nervous system, and those which fall under the head of annunciative value of stimulus. The first comprises a constant and a variable; the former being the fundamental dynamic constitution of the nervous system in the species (smaller or larger zoölogical group), the latter, the intoxication condition of that system at any given moment (the two summing to the "functional condition" at that moment). The actual reaction of an individual will then be the resultant of one constant and two variables; and we might epitomize, paradoxically, the respective shares by saying that the fundamental dynamic constitution determines form of reaction, subject to the qualifying effect of a "predisposing cause" (intoxication condition), and the dominant rôle of the "exciting cause" (stimulus).

Fundamental dynamic constitution of nervous system. To the operation of this factor are referable those reactions (consequently also, the habitually preter-active reorientation proclivities necessarily precedent thereto) which are approximately constant in an individual from age to age, season to season, and especially those common to him and others of the same species (particularly of the same sex) or narrower or wider zoölogical group. Examples: The herbivorous and carnivorous habits, and a chain of habits tributary to the latter (lying in wait, chasing, seizing, killing). The essential characteristic of the reorientation-proclivities referable to this factor is persistence in space and time; over large zoölogical groups, wide areas, and (inferentially from fossil structure) extensive periods of geologic time. This factor is, thus, the element in animal reactions, which makes for stability.

Intoxication condition of nervous system. The most striking characteristic of animal (especially human) reactions is capricelike super-variability often defying successful prediction and leading to belief that they are "free," though they conform

closely to the law of the average.[1] Of late, fore-falling shadows of the deterministic hand on the wall, have led to much subtle trimming as to just how free, "free" is. Leaving that to those interested, apart from fatigue (in which diminished activity in unduly used mechanisms incidentally involves shift over to other mechanisms), nothing yet brought forward sufficiently explains this pre-eminent variability. But fatigue represents one case of a wider category, the intoxication condition of the nervous system, the sum total of the *chemical* forces acting on the organism. These embrace habitual and unhabitual products of metabolism, habitual saline intake; secretions of glands (sex, adrenal, thyroid, pituitary); products of micro-organisms (toxins, ptomaines, leucomaines); and the motley host of unhabitual chemicals called "drugs." The two-sided relation involved in emergence of consciousness explains similarity of psychic action of substances widely remote in composition; some intoxicants supposably increase or decrease nerve energy, some increase or decrease resistance, and some act simultaneously on both factors in the same or opposite directions, whence the possibility of all necessary combinations of effect.

To the operation of this factor upon the fundamental dynamic constitution are referable those reactions which vary from sex to sex and season to season; and many of those which in one individual vary from age to age, moment to moment, and from conditions, of health to those of disease. This factor is, then, the psychic variation-inducing factor, *par excellence*. The following comprise the principal variations in reaction type:

Variations from sex to sex: Everywhere observable (collection of instances in Darwin's *Descent of Man*). Throughout the vertebrates, the male exhibits mental attributes summing to aggressiveness, whether in combat or the sexual pursuit; whereas the female shows a relative backwardness, qualified by the periodic emergence of "mother-love." All psychic characters being more apparent the greater the mental development, these variations are well-marked in the human, and have given rise in all peoples to sexual-social customs and rituals reaching extreme elaborateness. These variations are explicable as the outcome of unisexual intoxication.

Variations from individual to individual: These are seen in all animals including man, in which in connection with greater brain development the "principle of individualization" attains most marked expression. Some are addicted to gastro-intestinal matters; others turn from these to respond to sex stimu-

[1] *Cf.* Morselli, *Suicide*. Yet if any act be "voluntary," suicide is that act.

lation; still others recoil from this to engage in its artistic or religious equivalent; while some react only to a particular congeries of stimuli ("hobby") entirely peculiar to themselves.

Reactions strictly personal to the individual and constant in him from age to age, season to season (etc.) are referable to his *Anlage* and mostly to the fundamental dynamic constitution of the nervous system.

Variations from age to age: Under this head come reactions in connection with dependence of offspring upon parents, protective concealment of defenceless young not cared for by parents; and the predominantly gastro-intestinal and motor interests of the young, in contrast with the sexual and later parental interests of the adult. These variations are referable to age variations in intoxication conditions, whether in the way of surplus or deficit, except where they are the outcome of imprintation ("experience").

Variations from season to season: Seasonally recurring ruttings, combats, migrations, abstinence from food on migration, are among a host of instances. These are referable to seasonal variations in intoxication conditions.

Variations from moment to moment: These are continually observed, especially in the young ("distractibility"). Most of these are in reaction to the perpetually shifting environmental stimulus-succession; but the hour-to-hour variations ("moods") into which the moment-to-moment changes are in-woven, are, in my experience, largely the outcome of shifting intoxication conditions.

Variations from health to disease: These range from restlessness of fever to wildest delirium or to deepest coma; from the normal feeling of well-being, to the delusions of grandeur of paretic dement or to the unrelieved gloom of the melancholic. Many of these variations are now recognized by medical writers as due to intoxications.

Finally, marked change in mental condition frequently follows critical discharges (colon lavage, ejaculation, abscess evacuation, etc.).

Stimulus. In what sense can kind and intensity of stimulus be said to enter as elements in determination of form of reaction and of reorientation precedent thereto? Any classification of stimuli from the standpoint of their reaction-evoking (reorienting and psychogenic) action will be incommensurate with classification based on their physical or chemical properties. This is exemplified by difference of reaction of different nervous systems to the same stimulus, the visual stimulus-complex, "cat" evoking promptest reaction from a mouse, none from a by-standing cow. Still, this incommensurateness

does not mean that reaction-evoking (reorienting, psychogenic) action of a stimulus is independent of its kind and intensity, but that these attributes enter as elements of effectiveness not by reason of their physico-chemical values, but only as they are necessary elements toward identification; toward Recognition, of the stimulus as one experienced before, as a member of a chained series, the remaining members of which are yet to come. Effectiveness of stimulus thus corresponds to an *œcological* value, the annunciative power. This value, however, bearing no direct and definite, but only an incidental relation, to the physico-chemical properties of the stimulus, reorienting (reaction-evoking, psychogenic) capacity of stimulus is not, properly speaking, a dependent function of stimulus at all, but is an adaptive acquisition (susceptibility) of the organism.

II. CORTICAL-PSYCHIC (CONSCIOUS) ACTIVITY

We now approach the psychologist's ultima thule; his holy of holies, presided over by Vishnu-Ego, to whom the brain submissively hands up all the data received by it from the sense organs, and Who, having duly deliberated thereupon, vouchsafes to Its neurone-minions Its behests. We shall, however, find no something-from-nothing device; but only that the final mechanical resultant discharged is not the mechanical resultant of the incoming forces of the moment alone, but a resultant into the composition of which enter, *as components*, certain other, reactive forces, contributed by the organism itself. These forces represent no necromantic thaumaturgy, but merely the static forces (partly rheostatic, partly reactive-dynamic) of the organism, accumulated through Preponderant Survival; forces which contribute to determine the only form of reaction of which its particular structure is capable.

Phylogenetic Divisions of Cortex. In man, the association tracts have wedged far apart (yet bridge over the hiatus between) two great brain segments, the sensory and motor areas. But as we descend the zoölogical scale, this intrusive wedge narrows up and practically disappears, and we reach the original state of approximation. Consequently, the cortex comprises the following regions:

Palæogenetic.
- Sensory area.
- Sensori-motor bridge.
- Motor area.

Neogenetic.
- Association tracts (as developed in higher vertebrates).

But "association tracts" being used in an unduly wide sense, the term *plasticity zone*[1] will hereafter be used.

Classification of Consciousness-Facies. The first requisite for understanding of mental action is trenchant distinction between mental process and mental content. There is but one mental process, embodied in the bioneuropsychodynamic relation: *In carbon molecules the heat incidental to conduction under resistance reacts upon the molecules in such manner that resistance is less for subsequent than for initial conduction; the intensification stage of this process in nerve tissue may surpass the critical, limen point, and involve the emergence of consciousness.*

There is but one mental content,—consciousness. Calling sensations, thoughts, feelings, "elements of mind," risks missing the *real* question: How comes it that consciousness, as introspectively observed, exhibits sometimes characters diagnostic for sensation (variations in quality and in intensity), sometimes those distinctive of feelings (fluctuation over the pleasantness-unpleasantness gamut), sometimes only the colorlessness of thought? The natural answer is: Differences in the aroused consciousness are referable back to differences in the evoking agencies; peculiar combinations of conscious characters are the outcome of correspondingly peculiar combinations among those agencies. Thus, consciousness *in forma* sensation differs in quality according as it is aroused by waves of auditory-nerve, optic-nerve (etc.), length; in vividness with the amplitude of the arousing waves. And the various sensation-facies assumed by the aroused consciousness are mainly outcomes of different combinations between these two independent variables.

What, now, do we *mean* by consciousness *in forma* sensation, *in forma* thought, *in forma* feeling; what are the underlying conditions entailing these introspectively observed differences in facies (of *content*); and what the respective dynamic rôles?

Consciousness *in forma* sensation: The pictorial, environment-representative ingredient in mind, exhibiting variations in quality and in intensity; exempt from laws of lapse and of fade;[2] office of dynamic-inseparable, introduction into composition of forces, of environmentally (spatially) interproportioned

[1] *Plasticity zone:* The locus of plastic neurones, embracing besides the association tracts, at least those parts of the sensory and motor areas, destruction of which obliterates memories.

[2] *Law of lapse:* Repetition of actions originally accompanied by thought involves progressive diminution (to disappearance) of that thought.

Law of fade: The rise-and-fall curve of feeling, parallels, as a dependent function, the rise-and-fall curve of the excitant intoxication; consequently, in the last analysis, the evolution-involution curve of the intoxicant organ or focus.

components; seat, sensory area (*pars*); palæogenetic; condition of arousal, critical (limen) value of relation between nerve energy and resistance in elastic molecules.

Consciousness *in forma* thought: That which lapses, the cognitive ingredient in mind; exempt from law of fade; office of dynamic-inseparable, reactive-dynamic effect on plastic nerve molecules, eventuating in "profiting by experience;" seat, plasticity zone; neogenetic; condition of arousal, critical (limen) value of relation between nerve energy and resistance, in plastic molecules.

Consciousness *in forma* feeling: The affective ingredient in emotion, exhibiting variations between opposite affective poles; exempt from law of lapse, subject to law of fade; office of dynamic-inseparable, intensification or minimization of reaction; seat, special affluents of motor area; palæogenetic or palæo-neogenetic; condition of arousal, critical (limen) value of relation between nerve energy and resistance, in intoxication-prepared elastic molecules.

Taxonomic value of lapse. But, it might be argued, sensations not lapsing (they are equally vivid however often experienced), are not consciousness at all; and, *de facto* we *think* not *in* but only *of* red. Lapsing is, however, nothing inherent in consciousness *qua* consciousness, but is an incident of the type of structure in which in man consciousness (*in forma* thought) happens to be chiefly manifested. And sensations not lapsing must be assumed to take rise in structure different enough to account for the radical difference in behavior.

The alternative view offers insuperable difficulties. If sensations are in a distinct category, they either react dynamically and so back-flow into the ebbing nerve-energy current, or do not so react and back-flow. If the former, possessing in common with thought, awareness, they agree with it in all important features except lapse (otherwise explicable), and the category falls. If the latter, there is no conceivable way they can satisfy the equation of the conservation of energy, and they are left without conceivable office.

The prime taxonomic importance of lapse consists, then, in its being an index of plastic structure. Lapse appears, indeed, not for itself, but incidentally, *en route* to profiting by experience; and thought lapsed, can, on occasion, revive as readily as it lapsed.

A. Pre-Compositional

Sensations. Sensations do not lapse by reason of biological necessity. Motor response appropriate to environmental happening can come about only as a sequel to accurate spatial orientation; consequently, only organisms can survive in which there exists *somewhere* antecedent (in the sensori-motor prog-

ress) to the composition zone, an area in which the intensities of the incoming stimulus waves bear to one another proportions representative of the proportions obtaining among the environmental objects among which the organism has to move. This area must be antecedent to the composition area, as only so can the stimulus waves carry into the coming composition of forces a series of kinetic values so interproportioned as to be space-representative. This area will be the sensation area. For, vividness of sensation being a dependent function of intensity of stimulus, the interproportions of the vividnesses of sensations simultaneously aroused, will be (secondarily) a dependent function of the interproportions of the initiating stimuli. But this interproportion (of light, shade, color, *e.g.*) being derived from, will be representative of, existing conditions and interrelations of the environmental content of the moment. The sensation area is thus an intra-cranial camera-obscura shield-mirror, in which is perceived the direction in which the environmental Gorgon is to be struck.

The sensation area is the last stage in the sensori-motor progress, in which the intensities of the stimulus waves bear to one another proportions representative of environment; immediately succeeding this stage, these waves enter into composition with the reactive forces of the organism, forces different in every zoölogical group and individual. But the interproportion representative of environment is carried forward from the sensation into the composition stage. For, interproportion of vividness of sensation being a dependent function of interproportion of intensity of stimuli, interproportion of dynamic-inseparables, and consequently interproportions of reactive-dynamic effect and of reductions in the denominator of Ohm's formula,—all are equally dependent functions of the intensities of those stimuli. Whence, environment-proportional sensation-values, being multiplied by proportionals, remain environment-proportional.

So much for the biological necessity for constancy of relation between cortical-psychic processes and environment; what neurological conditions meet the necessity? What must be the molecule-type in the sensation-yielding neurones? One offering:

Constant resistance upon successive conductions.

Relatively *high* resistance.

Only with resistance constant can vividness of sensation be a dependent function of intensity of stimulus. But constant resistance upon successive conductions means a molecule yielding temporarily under the reorienting effect of the latentized nerve energy (*in forma* sensation), but *without undergoing imprintation* and reassuming its original condition upon the cessation of the reactive-dynamic action; in short, an *elastic* mole-

cule. This is no contradiction of the main thesis; carbon molecules *may be* plastic, but even in the body many are not (cartilages yielding temporarily to pressure do not yield more readily and extensively, and the cornea transmits light not better but only equally well, time after time). And carbon molecules differing in degree of plasticity, accumulation will, according to the biological necessity, take place in the direction either of plasticity or of aplasticity.

Fechner's law: If 1, 2, 3 (etc.), units of energy fall successively on 1 sq. cm. of body surface, the energy on each constituent small square of the centimeter-square (consequently, on each peripheral sense organ), is the square root of 1, 2, 3 (etc.); and the impinging energy initiates a current as the square root of the energy radiated on to body surface by the environmental source of energy, unless sense-organ activity alter proportions. But heat in the electric circuit[1] being as square of current strength, if the current initiated be electrical, *heat-generation in the nerve circuit is directly as energy-radiation in environment* fulfilling the biological necessity for environment-representation in the cortex, and showing how extensity in environment becomes polarized in one plane, as intensity, in the cortical-psychic zone, entailing qualitativeness of consciousness (more or less) in correspondence with the biological advantage of limitation of decision, at each moment, to choice between alternatives. Comparing Fechner's with the electric-heat formula, the constant corresponds to the resistance. Two facts favor their identity,—they are multipliers of homologous quantities, the constant is invariable despite varying strengths of stimulus, resistance invariable despite varying strengths of current. Invariable for the same sense but varying from some senses to others, the constant should be a specific-energy sense coefficient, but peripheral or cortical? Sense-organ activity varying *pari passu* with stimulus, the constant not so varying, can hardly be peripheral (anyway, unless the 1:1 relation between energy-radiation in environment and heat-generation in cortex be tossed aside, the true constant is the square root of Fechner's). But, if a cortical coefficient, invariability of constant within the same, and its varying on change from some sense-realms to others, is intelligible as indicative of equivalence of dynamicity among cells of the same realm, and difference of dynamicity between cells of different realms, which

[1] .0009477 C^2rt British thermal units, C being current (amperes); r, resistance (Ohms); t, time (seconds). Varying inversely as cube of radius of conductor, the heat generated in neurones and neuro-fibrils can be no negligible quantity. Its destiny can only be *work*, physical or chemical, probably both in different proportion in different cases and species.

accords with experience that while stimulation of one sense is never appreciated in psychic terms belonging to another sense (visual impressions never heard), vicarious interchange may take place within the same realm in hysterical reversal of the retinal color fields (explicable as intoxication-alteration of resistances, entailing cross-deflection *en route* with ultimate projection of color fields on to unhabitual cells). The constant would, then, measure the resistance of the sensation zone.

Did Fechner's logarithmic function formulate the quantitative relation between psychic and physical worlds, consciousness could not be compared with energy, no form of which interchanges with others on a logarithmic basis of equivalence. But the function seems intra-psychic; an orientation of judgment acquired through estimation of distance and speed relations of moving objects. If an animal perceive an enemy approaching from 200 meters distance, successive fractional distances traversed and distance-remainders yet to be traversed, are continually being estimated. Estimated how? Through intercomparison of sensation-vividnesses varying in dependent relation with shifting energy-impingement values incident to approach or recession in line of sight. Such intercomparison may be either of absolute sensation differences, or of the proportionate increase (or decrease) between one sensation and its predecessor. Supposing now, sensation-vividness correlated increment for increment with cortical heat, the latter equalling total energy-impingement on body surface, from a series of ascertained sensation-vividnesses we can calculate the distances whence the respective impingements were radiated[1] and thus test the comparative accuracy of the two modes of comparison. That scale would be best in which sensation-differences are proportional to distances traversed. But energy-impingements being as the inverse squares of distance-*remainders*, environmental conditions preclude *any* scale from being so proportional. Whence this most instructive case of two-story adaptation, where a series of judgment units is superimposed upon and sifts out the series of sensation units; proving that mental parallelism of environment exists not for sake of such parallelism, but because in general parallelism corresponds to maximum adaptation.

The table shows that at 200—199, for both series, the sensation-difference is 0.01, the "just noticeable difference" for vision; whence, 200 marks the diverging point of the two series.

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[1]If with 200 meters distance, impingement (i) = 1, for any point, p, distant less than 200, $i = \left(\frac{200}{p}\right)^2$, and $p = \frac{200}{\sqrt{i}}$

Comparison of sensation-differences and distances traversed.

BOTH SERIES		COMPARISON BY EQUAL SENSATION-INCREMENTS OF 0.01			COMPARISON BY LOGARITHMIC INTERVALS		
Number of term	Distances traversed should be	Distance-remainders (ϕ)	Distances traversed ($200-\phi$)	Separate Comparisons per meter	Distance-remainders (ϕ)	Distances traversed ($200-\phi$)	Separate comparisons per meter
41	40	169.03	30.97	—	163.92	36.08	—
81	80	149.07	50.93	2	134.35	65.65	—
121	120	134.84	65.16	2	110.11	89.89	—
161	160	124.04	75.96	4	90.246	109.754	2
201	200	115.47	84.53	5	73.965	126.035	—
241		108.46	91.54	6	60.621	139.379	3
281		102.6	97.4	—	49.685	150.315	4
321		97.59	102.41	8	40.722	159.278	—
361		93.25	106.75	9	33.375	166.625	6
401		89.444	110.556	10	27.354	172.646	—
441		86.068	113.932	12	22.42	177.58	8
481		83.046	116.954	13	18.375	181.625	10
521		80.323	119.677	15	15.06	184.94	12
561		77.85	122.15	16	12.343	187.657	15
601		75.593	124.407	18	10.116	189.884	18
1001		60.303	139.697	26			
5001		28.006	171.994	125			
10001		20.	180.	625			
40001		10.	190.	3000			

Clearly only organisms can survive in which, where complicated distance judgments (animal, enemy, or both, moving) are in question, sensations do *not* enter for their face values into, but find *some* reactive coefficient of correction in, the composition of forces. For, the sensation-increment does not even roughly approximate distance traveled; the sensation difference per meter differs widely in different portions of the line of approach (200—199, 0.01, 20—19, 10.8); and the number of separate comparisons is prohibitory. Further, as no animal could continually bear in mind the initial 200—199 increment as a referendum, intercomparison must come to be with *some* later term. But the later term, comparison with which involves greatest accuracy, is the next preceding. Such comparison

cannot, however, be on the increment basis, the distance-value of the increment varying too widely in different portions of the line of approach (200—199, 1 meter for 0.01; 20—19, only 0.001 for 0.01). Consequently, intercomparison must tend toward proportionality; toward the logarithmic. But such continual referendum to the next preceding sensation-value as a basis applies to logarithmic comparison exactly the corrective required to make it closely approximate to distance traveled, as that referendum makes that term the initial member of a new series with corrected base. The logarithmic series is seen to cover the whole line of approach with a series of sensation-differences not accurately proportional to distance traveled, but near enough for practical purposes; the sensation-differences vary far less widely in different portions of that line; and the number of separate comparisons is practicable. But judgment being in animals more exercised on distance and speed relations of moving objects (enemies, prey) than on all else together, all discrimination of sensation will come to be similarly oriented; toward logarithmic comparison. But such discrimination will not continually be attained by re-decision, but consciousness will lapse to the optimum limen, the "just noticeable difference," the *judgment* unit, not the sensation unit.

Meeting this biological necessity are composite-photograph imprintations in the plasticity zone (all the "learning" machinery) ensuing upon the innumerable trial-and-error experiences; (and, *de facto*, the young often exhibit imperfection in distance judgment) reinforced by elimination of the unready and inexact, and eventuating in a race in which judgment approximates the logarithmic type.

But if this be accepted as good and sufficient explanation of Fechner's law, then, successful explanation of that law hitherto mysterious, reflects retrospective lustre on the hypotheses which enabled such explanation,—that nerve energy is electricity, and that sensation is correlated increment for increment with cortical heat.

What does logarithmic proportionality mean dynamically; what neuromolecular conditions operate to magnify the small sensation-increments into the larger hiatuses of the J. N. D.? Unless I give up all won so far, or assume a psychic-cortical correlation for the plasticity zone different from that assumed for the sensation area, I must reason: Discontinuity in sensation-discrimination means discontinuity in plasticity-zone heat-generation; but with C^2 and r increasing subcontinuously, the heat increases by small increments (retaining visual discrimination as the example, a, 1.001a, 1.002a, . . . 1.009a); and while the relation r not $> \frac{1}{C^2}$ must *somehow* obtain in order that

neurone A shall yield sensation a upon impingement a, nevertheless r and C being independent variables, appeal must lie to discontinuity-elements in the substrate.

Hypothesis. Sensation-discriminating neurones (or neurone-grouplets) of the plasticity zone constitute a step-by-step graded series, A, B, C, ... Z, each neurone of which can rob (via collateral afflux consequent upon preponderant resistance-reduction) the territories of the other neurones according to circumstances and within narrow limits; for current values making $C^2r =$ successively to a, $1.001a$, $1.002a$, . . . $1.009a$, neurone A holds its own as against B, but the value a $(1+.01)$ reached, B robs A, but holds its own as against C so long as the value a $(1+.01)^2$ is not reached, at which point C becomes able to rob B (also all other neurones). Motor connections of constituent neurones are peremptory in proportion to the robbing power of such neurones. Each neurone consists of molecules specialized (via optimum lapse and Preponderant Survival) in such manner that r not $> \frac{1}{C^2}$, but r and C being independent variables, that limitation of value cannot come about through purely physical causes, but for neurone A (specialized for sensation a), supplementary heat-values $1.001a$, $1.002a$, . . . $1.009a$, *some chemical* alteration (presumably slight isomeric) must occur involving absorption in intra-molecular work.

This explains the disproportionate "attention" and reaction to strong stimuli. Also, it accords with the biological advantage of mental shock in arousing attention (meaning dynamically, promptness in robbing other previously active tracts, thereby turning nerve energy and consciousness in the newly needed direction quickly).

Inheritability of "learning" structure. Sensation-discrimination thus seems a matter of trial-and-error ("learning") adaptation, reinforced by "selection" among carbon molecules differing in initial resistance and capacity for ultimate modification; in plasticity, that is. Romanes believed the question of inheritability of acquired characters would be settled in the psychic realm, if anywhere. But there seems no necessity for assuming such inheritability in that realm; and the Baldwin-Osborne-Morgan "factor," the subtlest device for getting something out of nothing in matters psychic I have ever met, is superfluous. The "factor" is (1) in each successive generation the young learn anew a survival-requisite special artifice the learning of which saved the parents to the breeding period; until such time as (2) variations in the same direction appear in the germ. But *can function outrun structure?* Bodily

structure, yes; plastic structure, no. "Learning" *does not come from nothing;* learning capacity is imprintation capacity which is a dependent function of the plasticity interrelation, the relation between initial resistance and ultimate modifiability. But equally with other brain structure, plastic molecules present in the parents can, on *any* theory of heredity, reappear in ("be transmitted to") and vary in the offspring which, as compared with the parents as regards degree of perfection of the plasticity interrelation, may be on a par with, inferior to, or superior to them. Pending contrary proof, however, these differences are variations strictly comparable with other variations ("spontaneous," *de facto*, and from unknown causes). But the "acquired" imprintation-improvements of the parents cannot, so it is said, be "transmitted." Waiving discussion of what I know naught about, here at least it is not necessary that they should be; for "transmission" of learning-ready machinery entails learning-readiness of offspring on occasion (the "variations in the same direction" are *already* "in the germ"). Thus incidentally and in a manner the reverse of the Baldwin hypothesis, the young of ready-learners of the artifice, re-learn it, thereby surviving in largest percentage to the breeding period and leaving largest percentage of offspring. And obviously the limit of the process is a readiest-learning race, eventuating in one which is *pre*-ready, not needing imprintation-improvement of the plasticity interrelation, one which is able to execute appropriate motor response the first time occasion demands. Several degrees in the approximation-series can be seen in young animals of different species.

Consciousness. The logarithmic function relegated to the distal side of the sensation-thought boundary, no objection remains to translating the "rigid correspondence" of page 63, into: Correlation increment for increment; and consciousness and the cortical heat-like energy fall, dynamically, within the limits of one kinetic species, the name of which may as well be consciousness as anything else. And Penultimate Philosophy will say: Consciousness is the dynamic factor (the cognitive integral) in "profiting by experience." The consciousness wave-series probably adjoins (or more or less overlaps) the lowest of the heat series, extending thence indefinitely downwards. This position is, dynamically, most striking as in keeping with the character of consciousness as a *compromise* energy; one (perhaps the only one) into which can be directly and readily compositioned all the diverse energy-types impinging upon organisms,—the molar-motion type (all mechanical impacts including olfactory); aerial sound pulses; the several etheric-vibration series. Conveniently analyzable as dynamic- and psychic-inseparable, it may well be that "in-

separable" should be *identical.* For, there being no antecedent indication as to what properties waves referable to hitherto unknown portions of the spectrum should have, awareness may come to be accepted as *de facto* inherent in these particular waves, as is expansion-induction in those of the termic series, and silver precipitation in the ultra-violet, no one property being any *more* unintelligible than another.

B. Compositional

Recapitulating, every moment innumerable waves initiated by sensory stimulation are surging up through tier after tier of afferent neurones. At every successive millimeter of the upward progress, the waves are divisible into two classes,—waves for which antecedently permeable routes exist in the transverse sensori-motor connections at that level, and waves for which the antecedently permeable route is upward. The waves making up the progressively diminishing residual of the second class come ultimately to be deflected into the sensation area, where increased resistance involves friction with consequent latentization of nerve energy beyond the limen point, whence emergence of sensation, followed by the passing on of the waves, space-representatively proportioned.

Like other nerve action, plasticity-zone action is merely repetition of what it has been in the past, with two differences,—differences the outcome of past imprintations, and differences due to variations in "functional condition" (intoxication, including fatigue). And it is just because such differences cannot be excluded, that the sketch to be given will depart from our actual experience of mental action. But now the constants in nerve action are under analysis. Plasticity-zone action falls naturally under: Route, and the three-faced indecomposable fundamental fusion-composition-reorientation process, the distinctive feature of such action.

Route. This is strictly a dependent function of the several resistances, and (variations in functional conditions apart) these are strictly such function of the imprintation conditions. Thus is explained how the "experiences" of the individual, though past, can still enter, as a present factor in reactions. For, route incidentally determines which among the myriad host of cortical cells shall be impinged upon and stimulated. Route thus determines *kind* of nerve energy liberated (so to speak). But imprintation conditions have nothing to do with the *amount* of nerve energy liberated, that amount being exclusively an outcome of accumulations via Preponderant Survival, and in present reactions varies *pari passu* with different intensities of the same stimulus.

Fusion. Fusion-focus:—Sufficient gradient accumulated,

the sensation waves penetrate the plasticity zone, everywhere along the antecedently permeable routes. Penetrate how far? Till they can penetrate no farther; till they impinge in the abutment area of a fusion focus, a rheostatic *cul-de-sac*, exit from which is impossible for the waves *as such*, and possible only for their fusion wave. How could such a "focus" come about? Take the most difficult case possible (rather impossible). Suppose a system of neurones, all unimprinted and all equally plastic, every neurone forming terminal arborizations round the cell of every other neurone; neurone A arborizing round B, C, D, etc.; neurone B round A, C, D, etc.; neurone C round A, B, D, etc. Then A arborizes round B and B arborizes round A, A round C and C round A, A round D and D round A, B round C and C round B, etc. In such a system, impinging energies will start waves radiating in all directions; stimulation of A, *e. g.*, will be followed by radiations out to all its terminal arborizations. But stimulation of the circum-arborized neurones (B, C, D, etc.), will start secondary waves radiating out from each such neurone to all its arborizations, thereby stimulating all those circum-arborized neurones to radiate tertiary waves,—till waves of higher order fail to evoke further energy liberation. But all those waves imprint; and some environmental happenings recurring more frequently than others, certain stimulus waves recur in particular combinations, correspondingly more frequently; whence, inevitable inequality of imprintation conditions. For, in the neurone system supposed, with incoming waves corresponding to the ingredient sensations, *a*, *b*, *c*, of a concept, *abc*, impinging on neurones A, B, C, the routes radiating out from all the neurones of the system will fall as regards the type of the resulting imprintation, into three classes,—*intra-focal* (A-B, B-A, A-C, C-A, A-D, D-A, B-C, C-B, etc.); *exo-focal* (A-E, B-E, etc.); and *extra focal* (E-F, F-E, E-G, etc.). And the imprintation types will be:

	Route A-B	Route B-A
1.	Primary waves generated in cell A under stimulation of incoming sensation waves (of A type).	Primary waves generated in cell B under incoming sensation waves (of B type).
2.	Secondary waves generated in cell A under stimulation of B-A primary waves.	Secondary waves generated in cell B under stimulation of A-B primary waves.
3.	Tertiary waves generated in cell A under stimulation of B-A secondary waves.	Tertiary waves generated in cell B under stimulation of A-B secondary waves.
	Etc.	Etc.

And what takes place along routes A-B and B-A takes place equally along other intra-focal routes. Very different is the imprintation along the exo-focal, and still more along the extra-focal routes.

Route A-E	Route E-A
1. Primary waves generated in cell A under stimulation of incoming sensation waves.	No primary waves.
No secondary waves.	Secondary waves generated in cell E under stimulation of A-E primary waves.
Tertiary waves generated in cell A under stimulation of E-A secondary waves.	No tertiary waves.
No quaternary waves.	Quaternary waves generated in cell E under stimulation of A-E tertiary waves.

Consequently, the routes interconnecting A, B, C, and D, being each time more frequently and more intensely imprinted, will with every new advent of the particular stimulus-combination, tend more and more to routes of lessened resistance, as compared with the exo-focal routes; whence, diffusion will ever be more limited to the forming focus, thereby accentuating the difference between the intra- and exo-focal resistances; leading to stricter limitation to the focus, etc. The depth of such imprintation and super-imprintation will depend both upon frequency of recurrence and degree of plasticity; and as the process in question involves the only element of survival value in plasticity-zone action, variations in plasticity will accumulate via Preponderant Survival.

A fusion focus is a physiological rather than an anatomical entity, and denotes merely a number of neurones which in the presence of certain particular sensation waves, act together, but any or all of which neurones may, in presence of other sensation waves, act in connection with any extra-focal neurones to form other similar foci. Consequently, the form and size of such foci may be anything; the focus may consist of closely contiguous neurones, or of neurones separated by the length and breadth of the plasticity zone.

Fusion :—The intra-focal routes are thus imprinted by primary waves, and super-imprinted by waves of higher order. The latter, generated in the same cells under stimulation of different character, may be assumed (pending contrary proof or emergence of discrepancy) to differ in *some* respect from the

corresponding primary waves.[1] But, if they differ at all, fusion seems certain. For, the plastic carbon molecules of route A-B, would, if imprinted exclusively and repeatedly by primary A waves, attain to an orbit form vibrating in unison with those waves. The subsequent action of a different wave type upon an orbit form so modified would tend to modify it in another direction, that leading to an orbit form vibrating in unison with those second waves. As, however, the molecules are not subjected exclusively and repeatedly to the action of the second waves, but are subjected alternatively and repeatedly to the action of the first and second waves, the only possible outcome, these waves being different, is the emergence of an orbit form vibrating in closest harmony with both the remoulding waves, one whose proper note (so to speak) is an harmonic of both their vibration rates.

Waves vibrating in unison with an orbit form will be transmitted without change. But where their vibration rate differs from that proper to the orbit form, more or less interference with consequent absorption will ensue. With absorption, however, heat will emerge and modify the orbit form toward unison (or closest harmony) of vibration rates. Except for unison waves, then, every transmission will be the outcome of reciprocal readjustment (of a vibrational composition, so to speak), orbit form modifying wave, wave modifying orbit form; the amount of change undergone by orbit form and wave, respectively, differing in different cases. But with every additional imprintation and super-imprintation, the proportion which a single new imprintation bears to the sum total of all past imprintations and super-imprintations is continually diminuendo. Consequently, the effect of such new imprintation upon an orbit form once established is likewise continually diminuendo; or the effect of orbit form on wave is continually crescendo. Whence, waves impinging on orbit forms perfectly attuned, either, if too remote in vibration rate (too dissonant) are not transmitted at all, or, if harmonically reducible, undergo absorption sufficient to admit of their vibration in harmony with the orbit form become relatively inelastic (or elastic within comparatively narrow limits) through repeated modification in one direction.

[1]Parenthetically, the orbit form of route A-B, being imprinted by the more intense primary waves, and super-imprinted only by the less intense waves of higher order, will, though modified to an harmonic ratio of B, remain fundamentally A in type; meaning that its vibration rate will approximate to unison with that of A type, but only to harmonic ratio to that of B type. Conversely, the orbit form of route B-A, will be fundamentally B in type, and only harmonic to A. And what takes place along routes A-B, B-A, takes place equally along other intra-focal routes.

But potential raised in the abutment area, discharge must occur somewhere; either the immediate final discharge from the focus, or a mediate discharge on to other intra-focal neurones. On the second alternative (as the more difficult case), discharge will be on to more or fewer neurones (discharge on to the same number only postpones the question). If it be said that discharge from the abutment area is primarily on to more neurones, and that reconcentration (discharge on to fewer neurones) first begins later, it comes to the same thing, the contention merely changing the question to this: Is the abutment area the place where for these particular waves, the principle of the increasing complication of paths reaches its limit, or does that principle first reach its limit in some post-abutment (but still intra-focal) area? The abutment area denoting the last point to which the waves can without change penetrate the plasticity zone, the question lacks standing ground unless it mean contention that harmonic reduction confers further penetrating power, a contention unimportant concerning which no presumption arises, pro or con. *The* question lies between:

Hypothesis. Discharge from the focus occurs with ever increasing diffusion, no reconcentration on to fewer neurones taking place.

Hypothesis. Discharge from the focus occurs via reconcentration on to fewer neurones.

On the first alternative, energy existent in the abutment area would be dissipated correspondingly to the diffusion; whence, its psychic-inseparable would dwindle in vividness, all the time persisting *in forma* sensation. But we observe the energy concentrate to reaction, and the sensations integrate to a concept; whence a presumption in favor of the second alternative, which, besides, corresponds with what obtains for the plasticity zone as a whole, where discharge via fewer neurones is demonstrable anatomically in the diminishing number as we pass down the motor side, and marked psychically by concentration of "deliberation" into "will" and "effort." Since, then, the law for the part cannot be presumed different from that for the whole, discharge via fewer neurones is legitimate hypothesis. But such discharge means simultaneous impinging of two (or more) different sensation waves, reduced to harmonic ratios, upon the same neurone. As, however, its orbit form cannot, at one and the same time, vibrate in unison with each wave separately, the only possibility will be the assumption by it of a vibration rate to which the rates of both (or all) the waves can be assimilated; a rate which includes all rates, a common (and inferentially the least common) multiple of the

separate rates,—which means vibrational composition to a fusion wave.

Composition of forces. This is involved in (is only another side of) fusion. Unless the conservation of energy be repudiated, the amplitude of the fusion wave must be assumed to represent the algebraic sum of the intensities of the components; and that of the super-fusion wave to correspond to such sum of all the waves undergoing super-fusion.

In reaction to these incoming sensation waves, and more especially to their fusion (super-fusion, etc.) wave, the plasticity-zone cells liberate a vast amount of energy, an amount the liberation of which in the past sufficed, *de facto*, to ensure motor response adequate to the exigency announced or presaged by the stimulus. But (variations in "functional condition" always apart) being compounded and super-compounded from equals, the final resultant will equal the predecessor resultant, whence exit having been possible for it, will equally be possible for the present resultant along the route imprinted by its predecessor in its exit. Consequently, discharge will ensue on to those motor mechanisms on to which that predecessor discharged, and discharge on to which sufficed to preserve the organism in former similar emergency, or to preserve its ancestors to the breeding period.

In different cases, the energies contributed by the organism bear very different proportions to the incoming energies; the former may predominate or be insignificant. The *biological* resultant (that of the composition of *all* the forces) will, then, be incalculable, not because it departs (as far, at least, as there is any evidence) from the path of mechanical exactitude, but incalculable by reason of our ignorance of the values to be assigned to the intra-organismal forces in any given case. Especially will these forces differ widely in amount, since each organism reacts not to the immediate happening announced by the stimulus, but to its coming sequels; to sequels which are members of œcological series which though having the initial term in common, nevertheless differ for every species or other zoölogical group.

Superfusion, Supercomposition. Fusion-composition taken place, the fusion wave following the exit route imprinted by its predecessor will undergo either final discharge from the plasticity zone, or reach some limit within that zone progress beyond which is impossible for it as such wave, another abutment area round which another (superfusion) focus will build up. The latter (more frequent) case underlies all those congruous serializations of associations constituting "trains of thought." In the new focus, fusion-composition is repeated,—and so on, till final discharge on to the motor mechanisms.

Reorientation. Is incidental to fusion-composition, the direction of exit of the fusion wave being given between its vibration rate and the imprintation conditions along the exofocal routes. The essence, dynamically, of reorientation, lies in redistributions in the kinetic field, the outcome of which is discharge on to motor mechanisms whose action saved in similar emergency. Reorientation and reaction are thus merely consecutive phases in one process, of which psychogeny is but another aspect.

But why do mechanisms already acting give over their activity upon the advent of stimuli arousing activity in other mechanisms, "attention" coincidently moving over? No organism can at the same moment "compose" (attend to, deliberate and decide upon) all the inthronging stimuli annunciative of happenings at that moment (*de facto* attention to plurality of subjects is difficultly acquired and is unknown among animals). Consequently, those organisms only can oppose to happenings appropriate response, which have accumulated selective receiving capacity. Through biological necessity and Preponderant Survival, such capacity must come to be oriented parallel with the œcological-importance scale of happenings. What neurological condition meets this necessity? Graduation of the antecedent-permeability scale parallel with the œcological-importance scale. Being variable, selective receiving capacity cannot lie in the sensation area (the locus of constancy of relations), but must be a plasticity-zone or feeling-tract matter. In the plasticity zone, it is the outcome of the fundamental dynamic constitution of the nervous system and of the imprintations (except to the not small extent to which intoxication conditions affect zone action). Stimuli entering the zone in order of œcological importance, if stimulus *a* enter neurone A, it will (supposing the stimulus continue acting) continue to flow along such tract as long as resistance along that tract remains less than along tracts entered by later stimuli (*b*, *c*, *d*, *e*). For, with resistance along A relatively minimal (otherwise *a* would not have entered it in preference to other tracts) and further lessened by the conditions involved in functioning (resistance-reduction consequent upon heat of passage of current), energy will inflow on to tract A from all collateral points. But with the advent of stimulus *f* into tract F in which *f* encounters less resistance than does *a* in tract A, direction of energy flow will become reversed, and F will rob A. Energy will, however, flow in from points at which potential previously stood highest, from tract just active, where, consequently, consciousness was most vivid. But with such drafting off from those tracts, consciousness there will first pale and then vanish, while it will as steadily rise to a bright glow (so to speak) in

the newly active area. Whence, we shall have just that shifting over from consciousness of the previous to that of the present subject, which we introspectively observe.

In the feeling tracts, pre-antecedent permeability is an outcome of graduation of intoxication conditions parallel with the œcological importance scale. Consequently, the supplanting of a former stimulus, by a subsequent one, is evidence of intoxication preparation (resistance-reduction).

C. Concurrent-Compositional

Some think mind originated in "some form of sentiency," a vague phrase for: Sensation plus feeling. With these ingredients, simply environed organisms would be in position to oppose to happenings appropriate response, since they would possess a space-orienting and a reaction-regulating (intensifying or minimizing) factor. Before the intrusion of the thought-wedge, reaction-sequences, *sine qua non* for existence, might come to be organized through adjustment of activity of the second factor, via accumulation of optimum gradations in intoxication conditions (the first factor, environmentally determined, is for every organism a fixed quantity and the point of departure). Neurologically, this means a vast number of neurones organized into a connected whole, with the resistances of the constituent neurones just such as to ensure optimum serial motor response. What would be the psychic condition along such tracts? Sensations and feelings being non-lapsing conscious states, the tracts should be conscious tracts. The gradual intrusion of the thought wedge will introduce comparatively little (though as we ascend the zoölogical scale, ever progressively more) modification; and that in the way of inhibitive regulation or control. For, the biological necessity remaining the same, unless motivation of the established reaction-sequence should come to pass over to the thought wedge, it will remain where originally organized. But the offices of feeling and of thought (rather of their dynamic-inseparables) differ radically. That of feeling is ensuring of certainty of performance of serially connected, and promptness of execution of suddenly necessary, reactions, thus contributing to survival in face of emergency. But the survival value of thought lies in the fore-warning accruing in connection with it, so long in advance of emergency, that there will be none. If the senses be arranged in order of the warning afforded of happenings, touch and taste afford warning only upon contact; smell and hearing cover a range of at most a few miles; sight, somewhat or much more; but the warning in connection with thought is without assignable limit. Which *means* this, and *no more*,—that the conditions involving emergence of thought are equally those involving

"profiting by experience," involving boundless possibilities of resurrecting experiences (and experience-fragments in new juxtapositions and combinations), to extemporize a color-scale (so to speak) against which to match any incoming stimulus-complex, to the end that its ground tint being ascertained, its coming sequels may be "inferred" from the *de facto* sequels of its ground-tint match, long in advance of their advent. Thought is thus *the* prophetic sense.

Analysis thus reveals between sensation and motor function *two* bridges, a sensation-feeling-motor and a sensation-thought-motor bridge, differing in antiquity and in office. Otherwise put, besides the two recognized types of nerve action, reflex and conscious, a third type exists, reflex-though-conscious or pleasantness-unpleasantness action, which is in no sense intermediate between the other two, being altogether differently (namely, intoxication) motivated.

Feelings not lapsing, the underlying molecule type must be assumed elastic. Feelings being a dependent function of intoxication, the predisposing dynamic-inseparable must be assumed to originate through intoxication-induced metabolic processes. But to this *basis* dynamic-inseparable comes to be super-added, reinforcing dynamic-inseparable arising through latentization of nerve energy radiated in from the sensation area, entailing intensification beyond the limen point and emergence of feeling. Were feelings wholly the outcome of such latentization, however, they would be dependent functions of sensations and consequently (composition not occurring in elastic-molecule tracts) dependent functions of intensity of stimulus.

It now becomes intelligible why feeling and thought are independent variables, some exhibiting predominance of one ingredient, some of the other; also why in the same individual, intense thought and intense feeling alternate, rarely coincide. The intoxication substrate also explains the feeling variations classified on pp. 81-83. In salmon, *e. g.*, paralleling seasonal development of the reproductive organs, inshore movement begins, the initial term in a months'-long concatenated reaction-sequence, each member in which involves progress of the organism to a point where it will encounter the next supersessory stimulus. What share has intoxication? Given a sequence too long-lasting and complicated for successful execution via reflex action, the only way all available energy can remain concentrated for months within narrow channels, without fatigue, is through an elastic-molecule system maintained in constant reorientation by continuous intoxication-induced generation of dynamic-inseparable, whereby one series of tracts becomes *temporarily* (cyclically, seasonally) reflex, in constant

pre-readiness for response to those stimuli which initiated in the parents motor responses, whose eventual outcome was successful spawning.

The only alternative is that the salmon "knows." What does this *mean;* to what commit one? That the salmon *thinks:* "perceiving my ovaries swell, I know my time approaches, I must turn inshore, enter the river, ascend to those gravelly stretches near the headwaters, there build my nest and spawn my as yet hardly begun eggs." Absurd! More important, *why* absurd; only by analyzing out justification for contemptuous repudiation can one advance toward precision of thought. Ascription to salmon of such thoughts is unwarranted because (1) only in the Columbuses of the human does thought attain to fore-triangulation of a mental chart whereby migration *per saltum* across a hiatus can be steered. Because (2) such thoughts involve appreciation of interrelation of organism and environment amounting to self-consciousness which is an outcome of introspective observation of the sequential relation between sensations and mental operations, and between those operations and motor (glandular, etc.) response,— which observation sums to recognition that, psychically, body surface represents the locus of all points in the universe from which originate initiators of psychic ingredients (sensations) which form the first members of series of which feelings and thoughts are subsequent members; and that body surface represents, coincidently, the locus of the limit points to which can be followed out the sequential relation of feelings and thoughts to motor response. More briefly, body surface represents the limit points of sensation and "will," equally whether the universe be mind-constructed or only mind-reflected. But then self-consciousness cannot antedate phylogenetically the rise of introspection, the only distinctively human psychic trait.[1] Be-

[1]Psychologists make far too little of introspection, James merely saying it is "mysterious." Introspection offers no special difficulty. Thought emerges whenever the relation between nerve energy and resistance comes to exceed the critical limen point, *irrespective of the source* of the instreaming energy. For every superfusion focus, that source is some other part of the plasticity zone. But the superfusion consciousness comes, *de facto*, to include appreciation of the sources and relations of the instreaming energies; to include, *as superfusion content*, the consciousnesses which those energies simultaneously arouse or immediately precedingly aroused. The consciousness of simultaneous or immediately precedent consciousness is thus inherent in superfusion; every superfusion focus is potentially introspective. But introspection involves two complementary conditions, the "power of volition," and *sufficient continuance* of the observed and observing consciousnesses. Neglecting the first condition as *flatus vocis*, so long as environmental (especially escape and food) needs press so continuously and insistently as to necessitate biologically (and conse-

cause (3) "attention" could not be maintained for months unwaveringly concentrated upon efficient performance of the details of the migration. And because (4) did the enormous amount of energy expended pass through the plasticity zone, imprintation on a phenomenal scale must occur. But in species migrating year after year, following cessation of the breeding intoxication, the individuals revert to seek-food reactions, contra-indicating phenomenal imprintation. In such species, too, and also where all the parents die before hatching, the young perform, untaught, the first migration as well as the old; whence here, profiting by experience (plasticity-zone action) does not enter. From the foregoing, then, migration cannot be thought-motivated.[1]

Allocation. Not lapsing, feelings cannot take rise in thought tracts; independent of intensity of stimulus, they cannot belong in the sensation area. Were feelings in *any* case "in the sensation," that case would be the preputial dermal sensation; yet even here, pleasure is not "in" the *sensation*, as the latter is *always* present, pleasure on preputial retraction present only sometimes (erotism) and *post-ejaculationem* pain is "in the sensation."

Whence, inferentially neither is in it, but sensations fall, as regards pleasantness-unpleasantness relations, into two groups, —sensations tributary, in general, to pleasantness tracts, and sensations tributary, in general, to unpleasantness tracts. And the abrupt shift-over *post-ejaculationem* from pleâsurable to painful means de-intoxication alteration of resistances whereby sensation waves previously tributary to pleasantness tracts become tributary to unpleasantness tracts. The sensation is tributary during erotism to pleasantness tracts because such tracts are those involved in innervating evacuation (observable correlation between pleasure-increments and rhythmical pre-ejaculatory pulsations). Whence, pleasure fluctuates synchronously with the *motor* side.

Similarly, pain (a sensation, being equally vivid, however often experienced) is, in general, tributary to unpleasantness tracts, not because pain *qua* pain is necessarily unpleasant, but

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quently through Preponderant Survival to entail) the preter-lability of orientation finding psychic expression in ultra-distractibility, such sufficient continuance remains impossible. But when plasticity-zone development attains a stage admitting of fore-calculation covering a range far enough in advance of happenings to involve *leisure* (pastoral, agricultural stages), integration of mind has reached the introspection-possible stage.

[1] When my "Habits of Fishes" (*Amer. Journ. Psychol.*, XIII, 1902, pp. 408-25) was written, I had not recognized the existence and importance of the intoxication factor. That factor complements and completes the explanation there given.

because being the specialized danger-signal sensation, it must, in accord with biological necessity, be tributary to the escape (drawback, run-away, etc.) tracts. Pain is not always unpleasant. In slight dental periostitis, while never losing the pain quality, it is associated with pleasurable (lustful) tingle; an exaggeration, through inflammatory over-sensitiveness, of the frequent association of biting with lust. In severe grades of the periostitis, both this tingle and pain-unpleasantness are commingled in different degrees.

The close connection between feelings and the efferent side appears from the striking prominence of motor elements in those complex pleasantness-unpleasantness states termed "emotion," and from the relief of pent-up emotion through its motor expression; from the culmination of sexual pleasure coincidently with a motor act (evacuation); and from coincidence of the pleasantness gamut with high efficiency, and of the unpleasantness gamut with inefficiency (fatigue, inflammation, etc.) of the motor apparatus.[1] Such close connection suggests allocation to the "motor area or special affluents of it," which affluents are the "feeling bridge." Like the fusion focus, this bridge is (at present, at least) rather a physiological than an anatomical entity; but unless the preceding analysis is to go for naught, to this bridge corresponds the bulk of the cortical sensori-motor connections in vertebrates whose hemispheres are limited to the oldest basal convolutions (hippocampus, etc.). If, then, feelings be regarded as consciousness-facies taking rise in neurone-systems in close connection with the *efferent* (particularly the ad-muscular) apparatus, and as facies whose predisposing condition of arousal is intoxication and whose office is intensification or minimization of motor (or other) response, we come near a theory of feeling [2]

Dynamic interrelation of feeling and thought tracts. Can "feeling cause thoughts," in the unphilosophic argot of the day; meaning, can energy which in transit through feeling

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[1]Likewise other bodily organs; most striking with the musculature, because its innervation is associated with consciousness.

[2]Feelings *may* be naught else than consciousness arising in the motor area itself, consciousness of height of potential in (state of efficiency of) that area. This position (distal, in the sensori-motor progress, to "attention") would accord with the elusiveness of feeling upon introspection. Feelings could then be thought of (enter the composition of forces) only as modification of thought-*content*, the outcome of repeated experiences of the relative ease or difficulty of inhibiting their motor expression; and, *de facto*, paralleling the biological necessity, we are able to gauge the character and force of the coming movement in advance. But, tentatively, it suffices if the connection of feeling with the *efferent* side be accepted.

tracts has aroused feeling, subsequently pass into the plasticity zone and arouse thoughts? Introspection warrants an affirmative; repeatedly deliberation is influenced and decision warped by feeling components. More convincing, feelings are remembered, and memory is explicable only as an imprintation phenomenon. Feelings are remembered, then, in terms of their association values.

Can energy coursing in the thought watershed overpass the divide into the feeling watershed ("thoughts cause feelings")? This question can first be answered after introspective restudies with due consideration of the problems here raised, particularly that of the paramount importance of intoxication processes. For myself, the more I have come to realize the wide range of that influence, the more I trace in cases, in which formerly I should have replied to the above question unhesitatingly in the affirmative, intoxication changes underlying and accounting for the newly appeared deflection of energy flow. Therefore, I suspect the future will say: Failing intoxication-induced lowering of resistance, the feeling tracts as a class have a relative higher resistance than the thought tracts as a class; whence, to the extent that intoxication does not enter as a resistance-reducing factor, energy entrant from the sensation area will trend predominantly into the thought tracts, and to the extent to which intoxication does so enter, energy so entrant will trend predominantly into the feeling tracts. And the relative extent to which that energy passes into the two classes of tracts has been determined through accumulation via Preponderant Survival,—in the former case of increments in the direction of optimum plasticity, in the latter of increments in the direction of optimum intoxication conditions.

III. INFRA-LIMINAL NEURONES[1]

The infra-liminal ad-cortical and ab-cortical neurones must consist of molecules offering:

Constant resistance.

Relatively low resistance.

Resistance must be constant, otherwise sensation could not be a rigid dependent function of intensity of stimulus (and space-representation would be impossible); and, on the downward side, co-ordination would be impossible. Resistance must be low, at least to the extent of always being infra-liminal.

There is no reason to think this molecule type the outcome of lapsed consciousness; rather it is apparently phylogenetically older than the molecule type of the plasticity zone.

[1] *Infra-liminal: Permanently* subliminal.

ON THREE TYPES OF BEHAVIOR

The Mechanical, the Coercitive (Magic) and the Anthropopathic (including Religion)[1]

By Prof. James H. Leuba, Bryn Mawr College

In his dealings with the different kinds of objects or forces with which he is, or thinks himself, in relation, man has developed three distinct types of behavior. A concrete illustration will bring them before us more forcibly than an abstract characterization. A stoker in the hold of a ship, throwing coal in the furnace, represents one of them. His purpose is to produce propelling energy. The amount of coal he shovels in, together with the air draught, the condition of the boiler and other factors of the same sort, determine, as he understands the matter, the velocity of the ship. The same man, playing cards of an evening, and having lost uninterruptedly for a long time, might get up and walk around the table backwards in order of change his luck. He would then illustrate a second mode of behavior. If a storm threatens to sink the ship, our stoker might be seen falling on his knees, lifting his hands to heaven, and addressing in passionate words an invisible being. These are the three differentiated kinds of responses he has learned to make, the three ways by which he endeavors to make use of the forces about him in his struggle for the preservation and the enrichment of life. We may designate them as—

1. The mechanical behavior.
2. The coercitive behavior, or Magic.
3. The anthropopathic behavior, which includes Religion.

The mechanical behavior differs from the anthropopathic by the absence of any reference to personal beings. In the sphere in which it obtains, threats and presents are equally ineffective. It implies instead the practical—not the theoretical—recognition of a fairly definite and constant quantitative relation between cause and effect. If science is to be provided with an ancestor, and only with one, it should be this first type of behavior rather than Magic. For the moment the existence of the fixed quantitative relations, implicitly acknowl-

[1]A portion of a little book "The Psychological Origin of Religion", soon to be published by Archibald Constable & Co.

edged in the first type of behavior, is explicitly recognized, science is born. Magic separates itself, on the one hand, from the mechanical behavior by the absence of implied quantitative relations, and, on the other hand, from anthropopathic behavior, by the failure to use means of personal influence; punishment and reward are just as foreign to Magic as to mechanical behavior. As to the anthropopathic type of activity, it includes the ordinary relations of men with men as well as those with gods. One's frame of mind and behavior when dealing with a human person, especially if exalted far above us, resembles Religion so closely that it is proper to place them in the same class.

Mechanical behavior and Religion are, obviously, by far the most common and important modes of activity among civilized peoples, whereas in primitive culture the coercitive behavior (Magic) is everywhere in evidence and Religion may be practically unknown. As one ascends from the lowest stages of culture, Magic gradually loses official recognition. Among us, though it leads only a surreptitious existence, it has by no means lost all influence. The list of magical superstitions that have retained a hold among us would be found tediously long. A numerous class of them includes the gambler's methods of securing luck. So-called 'religious' practices may really be magical. The cross, the rosary, relics, and other accessories of Religion, acquire in the mind of many Christians a power of the coercitive type; that is, for instance, the case when the sign of the cross, of itself, without the mediation of God or Saint, is felt to have power; or when 'saying one's beads' is held to possess a curative virtue of the kind ascribed to sacred relics by the superstitious. And even when the symbolism of the sign of the cross, and the meaning of *Ave Maria* are realized, it happens not infrequently that signing oneself and saying one's beads are regarded as acting upon the Virgin Mary, Jesus Christ, or God, in the manner of incantation, *i. e.*, magically.

It has been the habit of most students of the origin of Religion to concern themselves exclusively with the origin of the god-idea, as if the belief in the existence of gods was identical with Religion. They have ignored its other essential components: the motives or desires and the feelings, as well as the means by which, in Religion, the gratification of desire is sought. But the limitation of the problem of origin to that of the god-idea is not entirely amiss. For there are neither specifically religious motives, nor specifically religious feelings. Any and every human need and longing may, at some stage or other, become a spring of Religion, and conversely the feelings and emotions met with in any form of Religion appear also in

non-religious experience. As to the practical means of securing the favor of the gods, it is agreed that they were at the beginning essentially the same as those men were already in the habit of using in their relations with their fellow-men. It is the Agent or the Power with which man thinks himself in relation, and through whom he endeavors to secure the gratification of his desires, which alone is distinctive of religious life. And so the origin of the idea of gods, though not identical with the origin of Religion, is at any rate its central problem.

In the preceding remarks, as also in practically all writings on the origin of Religion, it is assumed that the god-concept precedes, in the mind of man, the establishment of Religion. This opinion is, as we shall see, the correct one. But it cannot be taken as a matter of course. Actions may become established in other ways. Our first problem is to discover how Religion arose, and what psychological capacities and conceptions it implies.

A comparative study of the three modes of behavior is, after all, the shortest way of gaining a satisfactory understanding of the origin of Religion.

What are the abstract conceptions necessary to the establishment of the three modes of behavior?—There is usually little difficulty in determining what end any particular action is intended to secure. It is quite otherwise if one wishes to ascertain the nature of the power from which the desired effect is supposed to proceed. The philosopher, suffering from the illusion to which his class is subject, is in danger of imagining the presence of highly abstract notions where much simpler mental processes actually take place. A comparatively easy way of getting oneself disentangled from these high-flown interpretations and of ascertaining what is the intellectual minimum really involved in these types of behavior, is to examine them in the least developed men known to us, or, better still—if they are to be found there—among animals. Let us accordingly turn for a moment to animal behavior with the intention of determining what ideas of power, or of agency, are involved in their modes of action, and thus take a preliminary step towards the solution of our problem.

Apes, dogs, beavers, in fact all the higher animals, show by their behavior a 'working understanding' of the more common physical forces. They estimate weight, resistance, heat, distance, etc., and adapt their actions more or less exactly to these factors when climbing, swinging at the end of boughs, breaking, carrying, etc. I remember observing a chimpanzee trying to recover a stick which had fallen through the bars of his cage and rolled beyond the reach of his arm. He looked around, walked deliberately to the corner of the cage picked up

a piece of burlap, and threw the end of it over the stick. Then, pulling gently, he made the stick roll until near enough for him to get hold of it with his hand. This ape dealt successfully with physical forces. Towards animals and men, animal behavior is quite different. A dog will beg from a man; he will not beg from a ham suspended out of his reach. Towards animals and men, animal behavior is similar to that of men when dealing with invisible anthropopathic beings.

One may well believe that the inner experiences of animals differ in these modes of behavior as much as their external movements. The feelings and emotions which appear in a dog's intercourse with his master are of the same species, if not of the same variety, as those felt by man when he deals with his fellow-men and with superhuman beings. Certain highly gifted animals feel blame and approbation, independently of physical punishment or reward, and attach themselves to their masters with a devoted affection possessing all the marks of altruism. The higher animals do, then, without any doubt, practise both the mechanical and the anthropopathic types of behavior, but they exercise the latter only towards *actually present* persons or animals. We shall have to consider subsequently the significant psychological difference to which this fact points.

But, is there no trace in animal life of the coercitive behavior? I know of none, though some perplexity might be caused by certain reactions animals learn under the tuition of man. What shall be said, for instance, of a dog who has learned to raise its forepaws when he wishes to be liberated from confinement under circumstances making the person causing the door to open invisible to him? Is this magical behavior? There is certainly no quantitative nor any qualitative relation between lifting up the forepaws and the opening of a door, neither is there any visible continuity between cause and effect. That the dog's action is not determined, in this instance, in the same way as that of a magician, appears when it is observed that whereas the latter would perform the same magical rite in a great variety of external circumstances, the dog will seek liberation by lifting its paws only when in the particular cage in which he has learned the trick, or in one very much like it.[1] But more about this pres-

[1] H. B. Davis has this to say on the power of generalization of the raccoon, a very intelligent animal: 'When an animal [raccoon] is forced to approach a new fastening from a new direction, it is often as much bothered by it as by a new fastening. Nevertheless, in course of time the animals seem to reach a sort of generalized manner of procedure which enables them to deal more promptly with any new fastening (not too different from others of their experience).' 'The Raccoon: A Study in Animal Intelligence,' *Amer. Jr. of Psy.*, Oct., 1907, p. 486.

ently. It is not to be overlooked that without the interference of man, the dog would never have learned to perform this quasi-magical trick. This illustration serves, if no other purpose, at least to indicate how apparantly slight is the impediment which prevents the higher animals from setting up a magical art.

It may be a matter for astonishment that two complicated and effective modes of reaction are arrived at by animals in the absence of abstract ideas about forces. Yet, so it is; before any speculation on power, before any induction or deduction, before any abstract notion of the nature of spirit and matter, animals have learned to deal quite well with what we call physical and personal forces. How did they do it? The study under experimental conditions of the establishment of new reactions in animals reveals the process very clearly. Imagine a cat shut up in a box, the door of which can be opened by pressing down a latch. When weary of confinement the cat begins to claw, pull, and bite, here, there, and everywhere. After half an hour, or an hour of this purposive, but unreasoned, activity, he chances to put his paw upon the latch and escapes. If again put into the cage, he does not seem to know any better than before how to proceed, Yet, something has been gained by the first experience. For now he directs his clawing, pulling, and biting more frequently towards the part of the cage occupied by the latch. Because of this improvement he finds himself released sooner than the first time. The repetition of the experiment shows the cat learning to bring his movements to bear more and more exclusively upon the door or its immediate surroundings. Ultimately he will have learned to make just the necessary movement and no other. In this gradual exclusion of useless movements, the cat is guided entirely by results. The psycho-physiological endowment required for acquisitions of this kind involves no abstract ideas but (1) the desire to escape; (2) the impulse and ability to perform the various movements we have named; (3) an indefinite rememberance of the positon occupied when success was achieved, combined with a tendency to repeat the same movements when in the same situation.

The method illustrated above by which animals learn to deal with forces in the midst of which they live has a much wider range of application in human existence than is generally supposed. Man's fundamental mode of learning is also the unreflective, experimental, one in which frequent blind attempts and chance successes slowly lead to the elimination of ineffective movements. Would you convince yourself of the vastly exaggerated rôle ascribed to abstract ideas and to logical processes in ordinary human behavior, inquire how 'power' is

conceived of by those who use it. What is in the mind of the stoker when he thinks of the power of coal? What is in the mind of the gambler when he tries to coerce fate? What in the mind of the necromancer when he summons the shades of spirits? Nothing definite beyond a knowledge of what is to be done in order to secure the desired results and the anticipation of these results themselves. The stoker thinks of what he sees and feels: the coal, in burning, gives heat; the heat makes the water boil; the steam pushes the piston-rod, and so forth. Each one of the successive links in the chain is vaguely thought of by him as striving to bring about the following one. That is how he understands the coal-power. And what does the ordinary person know, for instance, about electricity? Simply what is to be done in order to start the dynamo, light the lamp, switch the current, and what the effect will be in each case, nothing more. The superstitious person, whether belonging to a primitive tribe or to the Anglo-Saxon civilization of the twentieth century, understands in no other than this practical way the forces he deals with. I remember the delight shown by an elderly lady when a brood of swallows fell down our sitting-room chimney. 'It will bring luck to the household,' said she. I did my best, patiently and in several ways, to ascertain the sort of notion the lady had regarding the nature of the power that was to bring about the fortunate events predicted, and also to discover her idea of the connection existing between the fall of the swallows and the exertion of the 'power' in our behalf. I had to come to the conclusion that there was no idea whatsoever in her mind beyond those expressed by 'swallows-down-the-chimney' and 'happy-events-coming.' These two ideas were in her mind directly associated. When I declared my inability to see the causal connection between the two, she complained of my abnormal critical sense! Nothing more than the immediate association of an antecedent with its consequent need be looked for in the mind of most civilized, superstitious persons, and, of course, nothing more in the mind of a savage. That is sufficient for practical purposes.

The words 'matter' and 'spirit' wield a very considerable influence among us; what do they mean to most of those who use them? Physical science ascribes either extension alone, or extension and weight, to physical substances. Non-material forces are, then, according to science, both spaceless and weightless. I will venture to affirm that not one educated person in a thousand is acquainted with this distinction. Most of the few who have known it have forgotten it. So that the words 'matter' and 'spirit' mean different things to the philosopher and to the layman. In the popular mind, if spirits

are not perceptible it is because the senses are not sufficiently acute. Spirits are here or there, diffused over wide areas or concentrated in narrow spaces. The average Christian, whatever he may say to the contrary, is, theoretically speaking, a materialist, and, I might add, a polytheist. Whatever matter and spirit mean to him, and they certainly have a substantial meaning, the distinction made by the philosopher is for him non-existent. The following facts may be of some interest in this connection. A few years ago, in a conversation with a shop-clerk, I happened to mention a lead coffin made hermetic with solder. He was shocked, and objected to a dead body being shut up in a coffin of that description because it prevented the escape of the soul. This man had had an ordinary grammar-school education. Here are two quotations taken from answers of American College students to questions requesting a description of their idea of God. It should be added that the questions were given only to classes which had not yet taken up, or were just beginning the study of philosophy. 'God, to me, is a being of flesh and blood, for without this form he would seem unnatual and unsympathetic as our leader.' (Female, twenty years old.) 'I think of God as real, actual flesh and blood and bones, something we all shall see with our eyes some day.' (Male, twenty-one years old.) Together with these, and from the same classes of students, came a great number of very different answers; for instance this, 'God is an impersonal being. . . . I think of him as the embodiment of natural laws.' Descartes's conception may serve as a point of comparison: 'What the soul itself was, I either did not stay to consider, or, if I did, I imagined that it was something extremely rare and subtle, like wind or flame, or either, spread through my grosser parts.'[1]

If the philosophical distinction between matter and spirit is not ordinarily made, these terms express none the less a very definite practical meaning of prime importance: they mark the difference between forces that are not responsive to psychic influences (desire and emotion, ethical and æsthetic considerations) and those that are.

The trial-and-error method which serves to establish the efficient modes of behavior observed in animals, is so far-reaching in its possibilities that one might be tempted to regard it as accounting for the existence of Magic and of Religion. Were this theory tenable, the origin of the three modes of human behavior would have been brought back to one method of learning, the unreasoning, trial-and-error method. But, even a superficial consideration discovers insuperable obstacles

[1] *Meditationes*, ii, p. 10, Amsterdam, 1678.

in the way of this enticingly simple explanation, and compels the admission that magical art and Religion involve the operation of mental powers not required for the estalishment of the mechanical, and of the non-religious anthropopathic behaviors.

The first of the two differences I intend to bring out, is that if a particular action is to be learned by an animal, the gratification of the actuating desire must follow immediately, or nearly so, upon the performance of the successful act, and be frequently repeated at short intervals; whereas in man, as far as Magic and Religion are concerned, the results may follow quite irregularly upon the performance, often only long after, and, not infrequently, not at all. Had not the door opened every time the cat pressed the latch, but, let us say, only once every ten times, or, if every time, one week after the movement, he would never have learned to make his escape. No more would he have acquired the trick, had he not been placed in the cage repeatedly and at short intervals. An interesting instance of the gradual undoing of a habit in consequence of the absence of the sensory results for the sake and under the guidance of which the action had been learned, is reported by Lloyd Morgan.[1] He had brought up in his study a brood of ducks. They had had a bath every morning in a tin tray. After awhile, the tray was placed empty in its accustomed place. The ducks got into it and went through all their ordinary ablutions. The next day, they again enjoyed the missing water, but not as long as on the first day. On the the third day they gave up the useless practice of bathing in an empty tray.

In three days ducklings eliminate a habit which has become useless, whereas generations after generations of men have gone through innumerable, time-wasting, often costly and painful ceremonies for results rarely secured, and, as we think, never directly secured by the magical or the religious ceremonies themselves. There is here a curious point of psychology: animals establish habits under the guidance of immediate results while man develops the magical art and Religion *despite* the usual absence of the results sought after. The very possibility of deceiving himself reveals the superiority of man over animals, for self-deception requires a degree of independence from sense-observation, a capacity of constructive imagination, a susceptibility to auto-suggestion, not to be found in animals. That the first glimmer of these capacities should have plunged man in the darkness of primitive Magic and Religion, and made him the ridiculous fool he appears to be by the side of the matter-of-fact, intelligent animal, is, however, a very striking and singular fact.

[1] C. Lloyd Morgan: *Introduction to Comparative Psychology* (The Contemporary Science Series, 1894), p. 89.

If the constant and immediate appearance of the desired results does not seem necessary to the establishment of Magic and Religion, it should not be thought, however, that these arts are altogether useless. On the contrary, they are, even independently of the results at which they aim, of a most substantial value to the cause of individual and social development. Let it be said first concerning the expected results that they happen more frequently, perhaps, than I may have seemed to imply. When, for instance, the rain ceremonies are performed during a spell of dry weather, success, more or less distant, always crowns the efforts of the magicians: the rain does come and the earth does bring forth its fruits. The ceremonies for the healing of disease are often followed by the recovery of the patient, however absurd the treatment may have been. One should not forget, in this connection, the considerable effect of suggestion upon the credulous savage. Many cures are, no doubt, performed in this manner by the medicine-man. Davenport, speaking of tribes of Puget Sound, says: 'Their cure for disease consists in the members of the cult shaking in a circle about a sick person, dressed in ceremonial costume. The religious practitioner waves a cloth in front of the patient, with a gentle fanning motion, and, blowing at the same time, proceeds to drive the disease out of the body, beginning at the feet and working upward. The assistant stands ready to seize the disease with his cloth when it is driven out of the head! And they are able to boast of many real cures.'[1] A psychologist is not inclined to doubt the report of Curr, that among the aborigines of Victoria persons who knew themselves to have been devoted to destruction with magical ceremonies have pined away and died,[2] nor that of Howitt, who, alluding to the habit of the medicine-men of certain tribes to knock a man insensible in order to remove the kidney fat for magical purposes, writes, 'In the Kurnai tribe men have died believing themselves to have been deprived of their fat.'[3]

But the intended results form only a part, and that perhaps not the most important, of the gains to be credited to the practice of Magic and Religion. The most noteworthy of these unsought by-products are:— (1) The gratification of the lust for power. The Magician and the Priest are mediators between superior, mysterious powers and their fellow-men.

[1] F. M. Davenport; *Primitive Traits in Religious Revivals*, Macmillan (1905), p. 36: quoted from the Fourteenth Annual Report of the [Amer.] Bureau of Ethnology, p. 761.

[2] E. M. Curr: *The Australian Race*, iii, p. 547, as quoted by Frazer, *Golden Bough*, 2nd ed., i, p. 13.

[3] A. W. Howitt: *The Native Races of South-East Australia* (1904), p. 373.

The sense of mastery over, or communion with, these powers, and the respect and fear with which Magicians and Priests are regarded, are, of themselves, almost sufficient to keep up these practices. (2) Both these modes of behavior, but especially Magic, appeal to the gambling instinct. All men crave excitement; the savage is no exception. In the daring game in which the rain-maker or the disease-healer engages, the high tension of the gambling-table is, to a certain extent, present. (3) Less obvious, perhaps, than the preceding advantages, but not less valuable, is the general mental stimulation induced by Magic and Religion. Magic is the great social play of the savage. If animal plays serve a highly valuable purpose in affording practice in sense-observation and motor-co-ordination, Magic makes its chief call upon the imagination; in this consists one of its most far-reaching values. It becomes a training for the achievement of those higher mental syntheses requiring the momentary disregard of the actual sense-impressions, from which it is so difficult to liberate oneself, in behalf of the accumulated experience of a whole life.

The second objection to the assumption that the trial-and-error method could have led to the establishment of magical and religious habits, arises from the inability of animals to act towards unperceived objects as if they were actually present. A dog never welcomes by gambols or licks the hand of an absent friend while Religion, and at times Magic, show primitive man in more or less systematic relations with powers he has never sensed. When the Shaman draws lines upon the sand, describes various curves with his arms, utters sundry incantations, he does not address a power he perceives, nor even one he has really seen, although he may believe that he, or some one else has seen it. That animals are moved to action by memories of past perception is, of course, not open to doubt. Their whole life is a long testimony to that ability. Any one will recall instances of chains of concerted actions indicating clearly, on the part of some one of the higher animals, domesticated or wild, the anticipation of a particular person, object, or event. What they never do, is to behave as if the remembered object was really present, though not sensed. H. Spencer, discussing adversely A. Comte's opinion that fetichistic conceptions are formed by the higher animals, relates the following observation concerning a retriever who had learned for herself to perform 'an act of propitiation.' She had associated the fetching of game 'with the pleasure of the person to whom she brought it', and so, 'after wagging her tail and grinning, she would perform this act of propitiation as nearly as practicable in the absence of a dead bird. Seeking about she would pick up a dead leaf, a bit of paper, a twig, or other small object

and would bring it with renewed manifestations of friendliness. Some kindred state of mind it is, which I believe, prompts the savage to certain fetichistic observances.'[1] So far the dog could go, but she could not have imagined the presence of an unseen being and behaved toward him in the same manner. Another significant point is that the absent objects towards which animals may direct their actions are always, so far as one may judge, identical with those actually sensed by them at some time, *i. e.*, their behavior never shows that they have transformed, imaginatively, objects with which their senses have made them familiar. Whereas, man can not only believe in the presence of unseen objects, but he can also imagine beings never actually sensed by him, and behave towards them according to the traits and capacities with which he has endowed them.

There are observations on record which compel the qualification of the assertion, I may have seemed to make in the preceding paragraph, of a clean break between man and animals. Certain dogs are thrown into paroxysms of fear by peals of thunder, and run into hiding. Darwin relates how his dog 'full grown and very sensible,' growled fiercely and barked whenever an open parasol standing at some distance was moved by a slight breeze. He is of the opinion that the dog 'must have reasoned to himself in a rapid and unconscious manner, that movement without any apparent cause indicated the presence of some strange living agent, and that no stranger had a right to be on his territory.'[2] Romanes, in a short and interesting paper entitled 'Fetichism in Animals,'[3] after reporting the preceding illustration, relates this observation touching a remarkably 'intelligent,' 'pugnacious' and 'courageous' dog. 'The terrier [Skye] in question, like many other dogs, used to play with dry bones, by tosssing them in the air, throwing them to a distance, and generally giving them the appearance of animation, in order to give himself the ideal pleasure of worrying them. On one occasion, therefore, I tied a long and fine thread to a dry bone, and gave him the latter to play with. After he had tossed it about for a short time, I took an opportunity, when it had fallen at a distance from him, and while he was following it up, of gently drawing it away from him by means of the long and invisible thread. Instantly his whole demeanor changed. The bone which he had previously pretended to be alive, now began to look as if it really were alive, and his astonishment knew no bounds. He first approached

[1] *Principles of Sociology* (3d edition, 1885), i. Appendix A, p. 788.
[2] *The Descent of Man*, 2d ed., i, p. 145.
[3] *Nature*, xvii (1877-1878), pp. 168-169. Comp. Lloyd Morgan, *Introd. to Comparative Psychology*, p. 92 ff.

it with nervous caution as Mr. Spencer describes, but as the slow, receding motion continued, and he became quite certain that the movement could not be accounted for by any residuum of the force which he had himself communicated, his astonishment developed into dread, and he ran to conceal himself under some articles of furniture, there to behold at a distance the uncanny spectacle of a dry bone coming to life.' Certain instances of instinctive fear of harmless things may help to interpret the preceding observations. G. Stanley Hall mentions a little girl who would scream when she saw feathers floating through the air. To keep another child in a room, it was sufficient to place a feather in the keyhole.[1]

Shall we hold that these animals interpreted the unusual experiences reported above as the work of hidden beings of the kind known to them, or shall we agree rather with Lloyd Morgan, Romanes, Spencer, and others, in thinking that their behavior indicated merely surprise, astonishment, and fear at the unexpected movements of familiar objects? That explanation is probably sufficient. The failure of an object to fit in with the psycho-physiological attitude of expectation which past experience has taught us to assume brings about the sudden disturbance called surprise, astonishment or fear. It is in subtance what would happen to any person if, on opening his bed in the dark, his hands came in contact with some object concealed in it. Personalization of the unexpected object is not necessary to cause fright. And yet, who shall say that in none of these instances is there anything corresponding to the anthropomorphic interpretation of natural events so common among men of low culture? Does not the growling of Darwin's dog indicate as much? It would seem to me an unjustifiably dogmatic assertion to affirm that no animal can think of thunder as caused by a being like those with which his senses have made him familiar. Were he to do so, he would do as the savage who projects his ordinary notion of animated beings behind inanimate phenomena. Creative imagination is not any more required for such an interpretation than for the belief in survival after death when it is suggested by apparitions in dreams or trances. It is quite in point, at any rate, to affirm that man and beasts are much nearer to each other, regarding the possibility of interpreting animistically certain striking natural events, than most people are willing to admit.

The most significant difference between men and animals is not found in the fact that animals may be unable to interpret animistically certain striking natural phenomena—an opinion

[1] *A Study in Fears*, *Am. Jour. of Psy.* (1897), viii, p. 166.

open to question—but in their inability to *fix* by means of communicable signs any fleeting animistic interpretation which might chance to cross their mind. Without the advantage conferred by speech upon even the lowest savages to hold, clarify, keep alive, and bring to fruition impressions of this evanescent nature, I do not see how a stable belief in animism could have been established. The decisive rôle played by language appears forcibly when one considers the part it takes in introducing dream experiences into waking life. The baffling evanescence of dreams caught sight of on awakening is familiar to every one. Unless clothed in linguistic form they are soon completely lost; verbal expression makes them part and parcel of our mental possessions.

Animals practice the mechanical and the anthropopathic behavior but, the latter, only towards beings present to their senses. Religion—anthropopathic behavior towards unperceived and, in lower religions at least, always personal powers—and Magic—the art of making use of an impersonal, non-mechanical power, to coerce things or persons—are found only in man. This fateful difference between animal and man is due to the latter's relative independence from slavery to sense-impressions. He is able (1) not only to sporadically imagine unseen personal agents but, thanks to speech, to keep their existence in mind, and (2) to establish and maintain practical relations with these imagined unseen beings, as well as to make use of suppositious impersonal forces, in the face of delayed and of oft-repeated failures on their part to produce what is expected of them.

A CRITIQUE OF PROFESSOR WIRTH'S METHODS OF MEASUREMENT OF ATTENTION

By L. R. GEISSLER

I have been working, for the past two years, upon the problem of the measurement of the degree of attention; and I have sought to approach the problem in the way that has already led to success in the case of intensity of sensation, by the assignment of numerical values to introspectively differentiated degrees of clearness. The problem has already been attacked by experimental psychologists, and notably by Professor W. Wirth, of Leipzig. Wirth's methods are entirely different from my own: but his general formulation of the problem, as that of measurable degrees of clearness, is the same. As I have found Wirth's articles both difficult and obscure, and as my criticism of his work must be given with some fullness if it is to be itself intelligible, I have thought it well to devote a special paper to the discussion of his contributions to the subject.

There is a difficulty at the outset, due to the fact that Wirth expresses himself only incidentally as to the relation of the field of attention to the field of consciousness. Wundt has made us familiar with the distinction between attention and inattention, apperception and perception, inner point of regard and inner field of regard. Wirth, on the other hand, appears (although I wish to speak on this point with great caution) to identify the range of attention with the range of consciousness. He aims to give "a precise determination of the range of simultaneous mental processes."[1] The title of his article in the Wundt *Festschrift* is "Zur Theorie des Bewusstseinsumfanges und seiner Messung." He speaks of the "Versuch einer Wiedergabe des gesammten Bewusstseinsumfanges."[2] Phrases like these seem to show at least that he is concerned with a total simultaneous consciousness, with both focal and marginal processes, even if they do not in themselves show that he draws no definite distinction between centre and periphery of consciousness. But we find, later, a passage like this: "the mere determination of the possible number of (tachistoscopically) isolated elements in maximal clearness does not at all show how high the absolute clearness-degrees may rise, or how many objects of the same clearness may be added to the average number of five isolated single objects, provided that all of them together form a familiar, associatively related whole."[3] Or again: "our measure of range really presupposes only a uniformly graded and highest possible average clearness of the whole complex.[4]" Here we have, appar-

[1] *Philosophische Studien*, XX, 489.

[2] *Op. cit.*, 493.

[3] *Op. cit.*, 524. "Durch die Feststellung der möglichen Zahl derartig (tachistoskopisch) isolirter Elemente in maximaler Klarheit ist jedoch noch gar nichts darüber bestimmt, wie hoch sich nun die absoluten Klarheitsgrade belaufen bezw. wieviele Objekte noch in der gleichen Klarheit wie bei ca. 5 isol irten Einzelobjekten zu diesen hinzutreten können, wenn sämmtliche Objekte zusammen ein geläufiges, unter sich associativ verbundenes Ganzes ausmachen."

[4] *Op. cit.*, 524. "Zunächst ist ja eigentlich nur die gleichmässig abgestufte und im Mittel möglichst erhöhte Klarheit des gesammten Complexes zu unserer Messung des Umfanges vorausgesetzt."

ently, an identification of attention and consciousness. And this inference is borne out by a passage in which Wirth discusses the relation between the apperceptive and perceptive regions of a simultaneous consciousness, and declares outright that there is no sharp difference between the two, but that on the contrary there are a number of transitional stages as we pass from the higher to the lower level. It is evident, he says, to unaided introspection "that the contraposition of an apperceptive and a perceptive region is not to be thought of as a mere dual division of consciousness. Within the simultaneous whole, several stages of attention and of clearness may always exist side by side, according as at any moment a larger or smaller number of unitary complexes forms the immediate experience of the subject. Although under certain conditions, which favor a kind of dual division, it is possible that a region, to which a fairly uniform attention is given, may be opposed to a 'background' or 'periphery' of consciousness, to which attention is as uniformly denied, nevertheless the concept of apperception ordinarily denotes a general direction of the process which goes on over the whole field, though at different places with unequal completeness."[1] Apperception is thus, as it seems, simply a general tendency to conscious self-realization, common to all the contents of a consciousness, but carried farther in the case of certain mental processes than in that of others simultaneously present. The antithesis of 'dunkel bewusst' and 'klar bewusst' practically lapses, as antithesis, and the difference between perception and apperception becomes merely a matter of degree. If it is objected that, even in Wundt himself, this difference has never been anything more, we reply that while the objection may be formally sound, in terms of a strict definition of the conscious states known as clear and obscure, nevertheless the distinction has played so important a part in the Wundtian system as to be, to all intents and purposes, a distinction of kind. This generic, Wundtian difference seems to be given up by Wirth, without defence or discussion; and we may add that Wundt himself, in his brief discussion of Wirth's experiments,[2] vacillates between the expressions 'range of consciousness' and 'range of attention' as if he too saw no reason to distinguish them.

The question at issue, however, is more than a question of personal belief or of an individual psychological system; it is a question of fact, of observation. That Wirth would have done well to face it as a question of fact will come out, I believe, in the course of the present paper. The above interpretation of Wirth's attitude is offered with all reserve, since his language—if I understand it aright—is not always consistent. I turn now to a consideration of his experimental investigations.

Of the three sets of experiments reported[3] the first is of a preliminary character, and is of no importance for the determination of degrees of clearness. In these experiments, a standard visual complex of 25

[1] *Op. cit.*, 493. "Hierbei zeigt sich vor allem noch, dass die Gegenüberstellung einer apperceptiven und perceptiven Region nicht etwa bloss als eine einfache Zweitheilung des Bewusstseins zu denken ist. Innerhalb des simultanen Ganzen sind jederzeit mehrere Stufen der Beachtung und der Klarheit neben einander möglich, je nach der grösseren oder geringeren Zahl von Einheitsbildungen, auf welche sich das Subject augenblicklich im unmittelbaren Erleben bezieht. Wenn auch unter Umständen in einer Art von Zweitheilung eine ziemlich gleichmässig beachtete Region einem ähnlich gleichmässig unbeachteten 'Hintergrunde,' bezw. einer 'Peripherie' des Bewusstseins gegenüberstehen kann, so bezeichnet doch der Begriff der Apperception mehr eine allgemeine Richtung des Processes, welcher innerhalb des ganzen Blickfeldes an verschiedenen Stellen in ungleicher Vollkommenheit durchgeführt ist."

[2] Phys. Psych., III, 1903, 358-360.

[3] Philos. Stud., XX, 1902, 635-659; Psych. Stud., II, 1906, 30-88; A. Kästner und W. Wirth, *ibid.*, III, 1907, 361-392, and IV, 1908, 139-200.

or less small geometrical objects was observed, with maximal attention, until all its elements had been clearly apprehended. During a short interval, usually of less than a second, the complex was slightly varied, in one or several different places unknown to the observer. Then it was tachistoscopically re-exposed for another fraction of a second, and the observer was asked to say whether, and if possible where, a change had taken place. Wirth's argument is as follows: "So far as, under these variable conditions, a correct judgment of difference remains possible, so far at least does the range of the simultaneous visual consciousness extend. But the difference necessary, at any given point, to arouse this correct judgment of difference, or in other words the differential limen under these special conditions of attention, is at the same time a measure of the clearness-degree which obtained at that particular point in the standard complex".[1] That is to say, if the change made at a certain place in the complex is noticed during re-exposure, then the object was in consciousness at some moment of the primary exposure; and the amount of change needed to arouse notice is inversely proportional to the degree of clearness of the mental content. The results show that, if the complex contained only 12 or 13 figures, any change in them could always be judged correctly; whereas, if the complex consisted of 25 figures, many errors occurred. With a momentary original exposure of a standard complex of 3 to 5 figures, changes in the re-exposure field were recognized with correspondingly greater or less certainty. Thus the results confirm and extend those of previous tachistoscopic experiments upon the range of attention proper.

After this preliminary investigation, Wirth comes back to the "problem of the completest possible determination of the range of consciousness",[2] which leads him more especially to "a quantitative determination of the simultaneously present clearness-degrees or degrees of consciousness of the greatest possible number of elements".[3] Nothing is said of the apperceptive and the perceptive regions; and it is only by a close examination of the experimental methods and results that we shall be able to decide whether Wirth has measured different degrees of clearness within the apperceptive region only, or whether he is dealing with the whole series of assumed transitional steps between the upper and the lower levels of consciousness. Wirth devised a special apparatus, whose main features are as follows. The field of vision for the left eye consisted of the funnel-shaped and uniformly illuminated surface of a glass cone 50 cm. in diameter at the base and 25 cm. high. The left eye was placed at the centre of the perimetrical field. The whole surface was divided into 6 concentric rings, the peripheral ring being again divided into 24 regions, the next into 20, the next into 16, the next into 12, the last into 8, and the inner disc into four regions. Of the 84 regions thus demarcated, 37 were selected for experimentation. The arrangement is shown in the Fig. By reference to this schema, the point of change could be easily identified. The conical surface was observed continuously,

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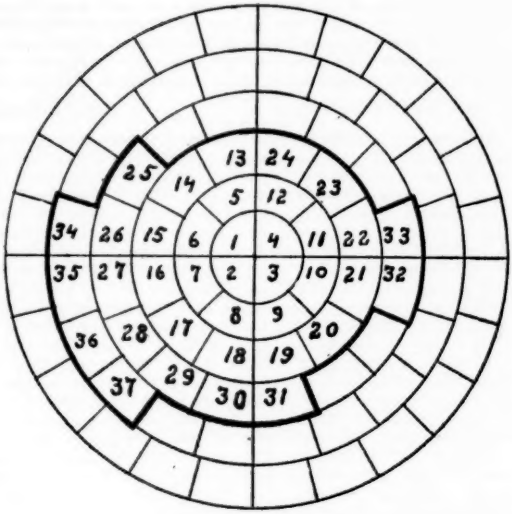
[1] Philos. Stud., XX, 593 *f.* "So weit sich unter den übrigen, bereits bekannten Bedingungen noch ein richtiges Unterschiedsbewusstsein bei einer solchen Variation mit Sicherheit einstellt, so weit reicht mindestens der Umfang des simultanen optischen Bewusstseins. Die Differenz aber, welche zur Erzielung eines solchen Unterschiedsbewusstseins an einer bestimmten Stelle nothwendig ist also die Unterschiedsschwelle unter diesen speciellen Aufmerksamkeitsbedingungen, ist zugleich ein Mass des Klarheitsgrades, welcher an dieser Stelle bei der Verteilung innerhalb des betreffenden Complexes herrscht."

[2] Psych. Stud., II, 30 *f.*; "die Aufgabe einer möglichst vollständigen Bestimmung des sog. Bewusstseinsumfanges."

[3] *Op. cit.*, 31.; "der quantitativen Bestimmung der gleichzeitig vorhandenen Klarheitsgrade oder Bewusstseinsgrade möglichst vieler Elemente."



with different distributions of attention, while by the manipulation of lenses and mirrors any one of the 37 regions could be gradually changed until its brightness was noticeably different from that of the rest. The first problem, then, was to establish the 'normal' differential limen for each region while the apex was fixated and the particular region was observed with maximal attention. If, for example, the brightness of the whole field of vision at a given moment was 174.5 photometric units, while the observer was attending to region no 2, and if the additional brightness necessary for the region to become just noticeably different was 29 photometric units, and the duration of the change was 6.75σ, then the normal limen for region no. 2 was $\frac{29 \times 6.75}{174.5} = .91$. The values for each of the 37 regions were similarly determined. The next question was to find out how the normal limen was changed by varying concentration of attention, and how the changes could be made to express or measure degrees of clearness. To answer it, Wirth divided the normal limen of any given region by its 'distraction' limen, that is, by a limen for the same region with a prescribed direction of attention. "A comparable value for the degrees of clearness (*Beachtung*) realized with a total distribution of attention over the various elements, each having its variously favorable position in the complex, may evidently be obtained without serious error if we divide the limen for maximal attention to a known region of change by the liminal value (for the same region) resulting from the method without knowledge, that is, from the method employed for the determination of the range (of consciousness)".[1] Or, more explicitly: "a more delicate differential or

[1]Philos. Stud., XX, 598. "Einen vergleichbaren Werth der Grade der Beachtung, welche für eine und die nämliche Gesammtvertheilung der Aufmerksamkeit mit

variational limen will correspond to a higher clearness degree; and the ratio of this limen to the normal limen for the same region as known in advance and made the object of attention will offer the greatest approximation to a comparable measure of degrees of clearness."[1] Thus, to return to our previous example of region no. 2; if we determine its distraction limen, while attention is distributed over the whole area, we find the value $\frac{30\ 3x25}{146\ 4} = 2.06$. The degree of clearness corresponding to the distraction limen 2.06 is then expressed by the ration 0.91:2.06. The clearness degrees for the other regions are determined in the same way. We thus obtain 37 different ratios, which are made comparable by reduction to the same basis of 100; *e. g.*, in the case of region no. 2, the proportion 0.91:2.06= 100:x results in the value 226 as the clearness index for a totally distributed attention. Wirth has determined in this manner the clearness indices of all the 37 regions for 6 different distributions of attention, and has presented them in 6 different schemata of the kind shown in the Fig., each region containing a ratio which represents its clearness for a certain distribution of attention. As to the psychological interpretation of these schemata, he says simply: "the value of these diagrams is quite independent of our psychological interpretation, which might, *e. g.*, be given as well in terms of purely dispositional factors."[2] Nevertheless, he certainly assumes that the numerical values of the schemata are, in some way and to some extent, definite quantitative measures of degrees of clearness. In order, then, to determine whether and in how far the results may be accepted as indications of differing clearness-degrees, we must examine the diagrams more closely. And we begin by considering them, so far as possible, from what we conceive to be Wirth's own point of view.

Wirth gives, first, a schema of minimal normal values,[3] which he employs for the calculation of the ratios with the six different distributions of attention. We should expect that these values would show normal characteristics as regards their magnitude, their relative frequency, and their spatial distribution. Now the lowest of them is 74, the highest 173, the general average 119, and the mean variation 19.3. The most frequent values (11) occur between 91 and 99, while 26 (or almost three-fourths of the 37) occur within the limits of 74 and 116, and the 11 highest values range between 123 and 173. The most regular spatial distribution is evinced by the four quadrants constituting the three innermost and complete concentric areas, and consisting of 24 regions. The left lower quadrant has the lowest average value, 89±5, and the right upper quadrant the highest, 126±7; the other two are nearly equal, 100±7 and 102±7 for the left upper and right lower quadrants respectively. However, if we look at the extreme values in each of the five concentric areas, we find that they range, as we travel in the centrifugal direction, between 91 and 107, 74 and 137, 87

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verschieden günstiger Stellung der einzelnen Elemente zu Theil werden, gewinnt man dann offenbar ohne grossen Fehler, wenn man den Schwellenwerth bei maximaler Beachtung der wissentlich variirten Stelle mit dem Schwellenwerth dividirt, der bei unwissentlichem Verfahren, also den eigentlichen Umfangsbestimmungen, gewonnen worden ist."

[1] Psychol. Stud., II, 31. "Dem höheren Klarheitsgrade wird hierbei eine feinere Unterschieds- bezw. Veränderungsschwelle entsprechen, deren Verhältniss zu der 'Normalschwelle' für die Veränderung des nämlichen im voraus bekannten und maximal beachteten Elementes die grösste Annäherung an ein vergleichbares Mass des Klarheitsgrades bieten dürfte."

[2] *Op. cit.*, 73. "Der Wert dieser Karten ist ganz unabhängig von unserer psychologischen Deutung, die z. B. auch im Sinne rein dispositioneller Verhältnisse erfolgen könnte."

[3] *Op. cit.*, 71, Fig. 8.

and 137, 97 and 150, and 123 and 173. The difference in the central area is the smallest (16), in the next area the largest (63), and in the other areas nearly equal (about 50). In spite of these irregularities, Wirth thinks that "there appears, after all, to be a sufficient degree of uniformity in the minimal values"[1] to warrant their use as 'normal' values. He therefore proceeds to divide them by the 222 distraction values, obtained with the six different directions of attention. It is obvious, however, that, if the normal values themselves show so little regularity, the 222 ratios partly depending upon them must be still less uniform as regards magnitude, relative frequency and spatial distribution. Besides, they are further affected by the fact that, while a certain part of the visual field is fixated, and another part observed with greatest concentration of attention, a liminal change of brightness may be expected in any possible region. Surely, such complications do not promise very satisfactory results. We find, in fact, that of the 222 values the lowest is 86, the highest 216. The extremes occur in the parts attended to as well as in those distracted from. For convenience of comparison we have arranged the figures in Tables I and II under the heading *Range*. The second table differs from the first in having the 9 high values above 200 eliminated. If, now, these

TABLE I

Attention to—	Total Field of Vision Range	Av.	MV.	Area Attended to Range	Av.	MV.	Area Distracted from Range	Av.	MV.
Total Field	108–284	165	30	108–284	165	30	—	—	—
Left Half of Field	86–181	131	23	86–181	130	24	88–178	131	20
Right Half of Field	106–186	136	17	110–173	136	20	106–186	136	16
Left Upper Quadrant	103–206	138	20	103–171	127	18	105–206	142	20
Left Upper Periphery	91–302	147	28	135	—	—	91–302	148	28
Fixation Point	93–162	123	15	—	—	—	93–162	123	15

TABLE II

Attention to—	Total Field of Vision Range	Av.	MV.	Area Attended to Range	Av.	MV.	Area Distracted from Range	Av.	MV.
Total Field	108–200	151	19	108–200	151	19	—	—	—
Left Half	86–181	131	23	86–181	130	24	88–178	131	20
Right Half	106–186	136	17	110–173	136	20	106–186	136	16
Left Upper Qu.	103–198	136	19	103–171	117	18	105–198	140	19
Left Upper Per.	91–192	141	22	135	—	—	91–192	141	22
Fixation Point	93–162	123	15	— —	—	—	93–162	123	15

[1] *Op. cit.*, 72. "So ist schliesslich eine hinreichende Gleichmässigkeit der Minimalwerte vorhanden."

values are really expressions of degrees of clearness, we should expect to find a marked difference in their distributions; the low values ought to be found in the parts attended to, the high values in the other parts of the field. Or, if unknown and occasional errors or the irregularity of the normal values be allowed for, we should still expect to find the *average* values for the parts attended to lower than those for the rest of the field of vision.

The tables show no sign of such a distribution. Wirth himself explains the disappointing character of the results as follows: "the effort of the special activity to bring about the right distribution of attention makes itself felt as a competitive factor. With distribution of attention over the whole field, it is probable that the lowering of the clearness values is for the most part due to this disturbing factor. But the distribution of attention to a certain region also carries with it the possibility that a higher clearness-degree may be assigned to some particular point which, under the circumstances, happens to be the object of maximal attention. [For these reasons] the arrangement of the values within the observed area gives no constant picture of a determinate clearness-relievo in the visual field under steady fixation."[1] Such an admission seems, however, to be fatal to the whole investigation. If the difficulties encountered were so great as entirely to obscure the results, then the experimental method employed is not applicable to the problem in hand.

The third set of experiments upon clearness-degrees is reported by A. Kästner and W. Wirth. Here recourse is had, not to just noticeable brightness differences, but to a plainly supraliminal brightness-change. The clearness-indices are expressed by the times of quickest possible reaction to the discrimination of change. Otherwise the experimental arrangements remain the same as in the previous work. The following groups of experiments were made: I *a*, simple reaction with knowledge of the region to be changed and maximal concentration upon it (to furnish a series of 'normal' reaction times corresponding to the normal brightness determinations in the former investigation); I *b*, complete reaction, without knowledge, and with attention distributed over some one quadrant; I *c*, the same with total distribution of attention; II, the same, with attention upon a region in the left upper periphery; III, selective reaction without knowledge, attention being totally distributed, and the reacting forefingers being assigned either to the right and left halves of the visual field, or else the one to a particular region and the other to all other regions. "It seems justifiable to assume that a proportional change in the reaction time will occur, if at all, then assuredly in connection with its dependence upon the degree of clearness with which the motive to reaction is apprehended."[2] The clearness-index is expressed by a number which is found by multiplying the reaction time of a given region, with a certain distribution of attention, by a fraction whose numerator is a constant (the average of the 37 normal values, 223.8σ) and

[1] *Op. cit.*, 87. "Indessen beginnt bereits die Anstrengung der besonderen Tätigkeit zur eigentlichen Verteilung der Aufmerksamkeit sich konkurrierend geltend zu machen. Bei der Verteilung auf das ganze Sehfeld fällt wahrscheinlich der Hauptanteil der Erniedrigung des Klarheitswertes dieser Störung zu. Doch trägt die Verteilung der Aufmerksamkeit auf ein Gebiet jederzeit die Möglichkeit in sich, einem beliebigen bei der Verteilung gerade maximal beachteten Punkt einen höheren Klarheitsgrad zukommen zu lassen, als ohne diese Verteilung. Die Art der Anordung der Werte innerhalb des beachteten Gebietes lässt keine Konstanz eines bestimmten Klarheitsreliefs im Sehfeld bei festgehaltener Fixation auffinden."

[2] Psych. Stud., III, 390. "Wenn irgendwo, so scheint nun gerade für die Abhängigkeit der Reaktionszeit vom Klarheitsgrade der Motivauffassung, die wir hier im einzelnen untersuchen wollen, eine solche proportionale Veränderung der zunächst von beliebigen Unterschieden der Raumlage modifizierten Reaktionszeiten angenommen werden zu können."

whose denominator is the normal average value (of 5 individual values) for a given region. Thus, if the reaction time for region no. 18 is 216σ, with total distribution of attention, while the normal average reaction for the same region is 221, then its clearness-index will be $216 \times \frac{223.8}{221} = 218$. Hence, with total distribution of attention, 218 represents the degree of clearness for region no. 18, independently of its position in the visual field.

The results of the normal reaction experiments are uniform. The times vary only between 200 and 245, and show a fairly regular distribution over the whole area. On the other hand, the results with various distributions of attention are so irregular that the authors are satisfied with the general comparison of the average values for the whole area, without, as a rule, giving due consideration to the large mean variations, or even distinguishing between the regions attended to and those not attended to. Their conclusion runs: "no matter how much the distribution of the times varies, over the whole visual field, for the different adjustments of attention, the general averages agree almost perfectly in cases of approximately equal practice; their mean is 282. This time is, therefore, in a certain sense the expression of a constant energy expended in the control of the whole perceptual field of possible motives to reaction,—an energy that, under the different distributions of attention, manifests itself in the time of preparation even for varying spatial positions; so that, on the average, a higher degree of expectation of a certain group of motives lowers correspondingly the preparedness of the observer for all the other possibilities."[1] It is difficult to see what relation such a conclusion can sustain to the problem of measuring either the range of consciousness or the degrees of attentional clearness. Some of the irregularities are, it is true, more or less adequately explained, as due to special circumstances, while others are termed simply "Zufälligkeiten."[2] The latter designation seems to apply, for instance, to the case of total distribution of attention, where the four regions of the most extreme concentric area contain not only the longest and the average reaction times of the whole series, but also the two quickest reactions of that particular series. However, it is not of vital importance that we should know the reason for the numerous irregularities. The fact that they occur at all, and prevent the appearance of the expected results, is sufficient to prove the inexpediency of the method. Indeed, in their closing sentence, the authors virtually admit the inadequacy of the investigation. "Further enquiry must determine how far a more detailed differentiation of the co-ordinations, with more than two possibilities of choice, may avail to furnish a more accurate representation of the clearness-relievo within any given group of motives to reaction."[3]

Thus far we have sought to review Wirth's investigations entirely from what we regard as his own standpoint; and our conclusion is

[1] Psych. Stud., IV, 163. "So verschieden als die Verteilung der Zeiten auf das ganze Sehfeld bei den verschiedenen Einstellungen der Aufmerksamkeit ist, so stimmen die Gesamtmittel bei ungefähr gleichen allgemeiner Uebung fast vollständig überein und ergeben im Mittel hier 282. Diese Zeit ist also gewissermassen der Ausdruck einer konstanten Energie zur Beherrschung des ganzen Wahrnehmungsbereiches möglicher Reizmotive, die bei den verschiedenen Verteilungen der Aufmerksamkeit in der Vorbereitungszeit auch in verschiedener räumlicher Aufteilung zur Geltung kommt; so dass die grössere Erwartung der einen Motivgruppe die Bereitschaft für die übrigen Eventualitäten im Mittel um ein Entsprechendes herabsetzt."

[2] *Op. cit.*, 148.

[3] *Op. cit.*, 200. "Es wäre nun weiterhin zu untersuchen, inwiefern eine weitere Differenzierung der Zuordnungen in einer mehrfachen Disjunktion ein genaueres Abbild des Klarheitsreliefs innerhalb eines Motivbereiches ergibt."

that he has failed to solve his problem. The difficulties of his method and the many factors that complicate his observations seem to obscure the real nature of his results and to rob them of any decisive significance. It may, however, be possible to give the results a new meaning by an interpretation from that standpoint which, in opposition to Wirth's, makes a sharper distinction between the apperceptive and the perceptive levels of consciousness.

There can be no doubt that Wirth's experiments have dealt with certain degrees of clearness. The question is whether they covered the whole range of a simultaneous consciousness, from the clearest region of apperception to the obscurest region of perception, or whether they were merely concerned with certain relatively small differences of clearness within the level of apperception itself. Wirth assumes that his low values represent a high degree of clearness. He has to admit, however, that his high values do not necessarily represent a low degree of clearness. They were due, at least in part, to distracting factors. While, therefore, the low values may possibly furnish a numerical statement of the clearness of processes on the apperceptive level, Wirth has been unable either to measure the clearness of processes on the lower level, or to prove the existence of his assumed transitional steps between the two levels.

Let us consider once more the liminal brightness-determinations obtained with various distributions of attention. The 222 values represent 100 actual ratios. Now Wirth certainly does not mean that each separate ratio measures a new degree of clearness, so that there are at least 100 stages between the two extremes of his experiments. If he had thought this, he would not have disregarded the large mean variations, and have contented himself with general averages. On the other hand, if his values do embrace the whole range of consciousness, and if they demarcate in a rough way some smaller number of intermediate steps, then we should expect them to fall into natural groups of greatest frequency, clustered around certain averages representing these steps, and thus to form a multimodal frequency curve. In this event, the number of such groups will indicate the number of intermediate steps, and the difference between their averages will be a rough measure of the size of the steps. But if we examine the total frequency distribution of the 222 values, irrespective of the various kinds of attention under which they were obtained, we find only the slightest indication of grouping between the values 90 and 160. The probable meaning of this we discuss later on. It is very significant that of the 100 different values only

2	occurred	7	times	(namely	130 and 144)
1	"	6	"	("	146)
7	"	5	"	("	103, 110, 119, 125, 137, 149, and 157)
12	"	4	"		
21	"	3	"		
44	"	2	"		

Although the figures vary, as was stated above, between the limits of 86 and 302, 90% of them (or 200 out of the 222) occur between 90 and 180; and, what is more, 75% (or 165) lie between 90 and 160. That there is no multimodal distribution may be easily shown by constructing a frequency surface, in which, *e. g.*, groups of 10 possible values are represented in each unit of the abscissa. The same fact is brought out in the following table:

Within the limits of	81	and	90	there occur	2	cases
	91	"	100	" "	10	"
	101	"	110	" "	26	"
	111	"	120	" "	31	"
	121	"	130	" "	32	"
	131	"	140	" "	26	"
	141	"	150	" "	30	"
	151	"	160	" "	21	"
	161	"	170	" "	13	"
	171	"	180	" "	12	"
	181	"	190	" "	6	"
	191	"	200	" "	4	"
	201	"	302	" "	9	"

Thus our expectation of finding natural groups of values is not realized. We find instead that the 222 (minus the 9 extreme cases above 200) group themselves around one general average of 136, with 75% of them clustering closely about 122. A similar distribution is found in Wirth's reaction times, as may again be seen from the construction of a frequency surface. This fact would seem to indicate that, during most of the experiments, the observer managed to maintain a fairly constant and fairly high degree of concentration, while during the remainder the difficulties of which we have spoken exercised a dominant influence upon the results. Such an assumption becomes still more plausible if we compare the frequency distribution of Wirth's reaction times with those of other experimenters upon the sensorial reaction. We may select for instance the results of Alechsieff as typical, since he also took sensorial or complete reactions to visual stimuli.[1] We call attention to the curves for his 4 observers A, K, F, and S. These observers were required to concentrate their attention maximally upon the stimulus. Their "Einstellung" thus differed as widely as possible from that required by Wirth. Nevertheless the distribution of their results is strikingly similar to Wirth's; so similar that the agreement can hardly be due to accident. It therefore seems fair to suppose that Wirth, after all, maintained a uniform concentration of attention upon the field of vision, and that his degrees of clearness—in so far as they are correctly indicated by his lower values—are only small variations upon the apperceptive level.

With this interpretation in mind we may come back to the slight indications of grouping between 90 and 160. Near our corrected average value of 122 there is, as has been said, a relatively large frequency of similar values. We notice further, small groups at 110 and 103, where we may perhaps assume slight rises in clearness above the common or normal level. We notice also small groups at the lower values 128, 137, 146, and 156, which in a similar way we may perhaps assume to represent slight decreases of clearness. The same thing appears on inspection of the reaction times. It is true that we are not here dealing with simultaneity in consciousness, since the values are taken from the different distributions of attention. And if separate curves are drawn for the separate distributions, they will be found to show both fewer and less marked deflections from their general apperceptive levels (especially in the case of the brightness-values, where all values above 160 should probably be eliminated). It would plainly be wrong, then, to lay any great emphasis upon these groups, in view of the extreme complications of Wirth's experimental conditions. They are at best, only very slight, and are mentioned merely in order to do full justice to the results.

[1] Philos. Stud., xvi, 1900, 1 ff.

All these considerations force upon us the conclusion that Wirth's numerical values, in as far as they may be admitted to measure degrees of clearness, refer to slight clearness variations on the apperceptive level only; that he has not measured the range of a total consciousness; and that he has given us no evidence of the existence of transitional degrees or steps between the apperceptive and the perceptive level. It may be added that, so far as can be seen without repetition of the experiments, Wirth's experimental arrangements were most unfavorable to a solution of his problem. All tachistoscopic experiments (and the conical field is, in essentials, tachistoscopic) tend to exaggerate the difference between the two main levels of consciousness. Under Wirth's conditions, the whole world of extraneous lights and colors was, objectively as well as subjectively, excluded from the observer's eye and mind. The difficulties of concentration of attention upon the various parts of the visual field, with maintenance of a constant fixation, must have required an almost hypnotic state of attention, in which the isolated tachistoscopic objects appealed to the observer with such exclusive force that the mental processes which would normally have occupied the perceptive level were driven towards or below the limen of consciousness. This is probably Wirth's justification for their neglect. But such an extreme and almost abnormal state of attention is, evidently, not the most favorable condition for the solution of his problem, in whatever form he meant this problem to be understood. It is too far removed from the conditions of daily life, where the most natural and frequent occurrence seems to be the more or less clear apperception of a single mental process or small unitary group of processes, upon a background of other, more or less obscure but still noticeable mental processes.

There are one or two other points that call for criticism. In the first place, practically the only observer, especially in the last two sets of experiments, was Wirth himself, so that there is no possibility of comparing results obtained from different individuals. The excuse offered is lack of time which, however, can hardly be accepted as valid. In the second place, comparison is the more necessary in this case since it is practically impossible to repeat the experiments anywhere except in the Leipzig laboratory. And yet they must be repeated, unless we agree that the methods are so faulty and so unpromising as to spare us the necessity of repetition. This, at any rate, is clear: that Wirth has failed to solve his problem. For, if we try to interpret his numerical values from his own standpoint, we find them meaningless; they are obscured or invalidated by complicating factors. And if we look at them from our own point of view, and eliminate into the bargain all doubtful cases, they appear to disprove his assumption of the existence of transitional steps between the apperceptive and perceptive levels. The main reasons for his failure seem to be uncertainty and ambiguity in the formulation of the problem; the impossibility of overcoming difficulties of observation; and the restricting conditions of his experimental arrangement, which was rather unfavorable than favorable to the type of consciousness under investigation.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

X. A Study of Retinal Rivalry in the After-image

By Alma de Vries and Margaret F. Washburn

If a stereoscopic slide be constructed having on each half a small square of differently colored paper, and if such a slide be looked at through a stereoscope from which the middle partition has been removed, double images of each square will be seen. If the distance between the squares has been properly adjusted, the two innermost images will coincide, producing a binocular image that fluctuates in color, displaying the ordinary phenomena of retinal rivalry. Now, when after prolonged fixation of the central square, the eyes are closed, negative after-images of the colored squares appear. The two images on either side show each in the color complementary to that of the square on its side of the slide; the middle image alternates between the colors complementary to those which alternated in the middle square seen on the slide. In other words, the retinal rivalry continues in the negative after-image. For instance, if the slide has a blue square and a red square on it, when seen through the stereoscope it will show three squares, a blue and a red one on either side and in the middle a square which is alternately blue and red; the negative after-image shows a yellow square in place of the blue one, a green square in place of the red one, and in the middle a square which alternates between yellow and green.

There is, of course, nothing remarkable about this phenomenon. Attention was called to it by Breese in his work on Inhibition. Working with a light of approximately constant intensity transmitted through red and green gelatine, he found (1), that "the rate of fluctuation in the rivalry of after-images is much slower than in the case of direct stimulation;" and (2), that the lengths of the phases varied on different days. Seven tests only were made, and the duration of fixation of the stimuli was one hundred seconds. Breese found also, as a result of his study of rivalry in general, that conscious effort increased the length of time during which the phases seen with open eyes lasted. But he did not observe what effect such 'voluntary' lengthening of phase had upon the phase of the corresponding negative after-image, and it was to this problem that we addressed ourselves in the work to be described.

Our methed of experiment was as follows: The observer looked through the stereoscope for forty seconds at a slide on which were two squares, one of green paper and one of blue paper, the saturated green and blue of the Bradley series. These squares were one cm. a side. The observer's hand rested on a telegraph key, by pressing down which the line drawn by an electric marker on a smoked drum was lowered. Time was registered on the drum by a Jacquet time-marker. The light was ordinary daylight, which of course varied in intensity; the experiments were always performed at the same time of day. During the forty seconds' fixation the observer recorded the time of the fluctuations of the middle image by means of the key, while the experi-

menter recorded on the drum the nature of the changes as verbally reported by the observer. The experimenter at the end of forty seconds gave a 'Now' signal, at which the observer closed her eyes and the fluctuations of the middle after-image were recorded in the same way as those of the original image. After completing such an experiment, a rest of three minutes was allowed. The after-image, it should be said, was observed as long as any trace of it remained. When three minutes had elapsed, the experiment was repeated, but this time the observer was instructed to make every effort to hold the blue color in the middle image during the period of actual stimulation. When the eyes were closed, the after-image was allowed to take its course as before without effort at control, the object being to see whether any lengthening of the blue phase by effort in the original image would affect the duration of the yellow phase in the after-image. Another three minutes' pause was then allowed, and a third experiment performed in which it was attempted to prolong the green phase in the original image. These three experiments were usually all that were demanded of the observer during a sitting, as the work was quite fatiguing. From six to ten such complete experiments, of three parts each, were made with each of the three observers. Later, the colors red and blue were used on the cards, five complete experiments being made with each observer. The observers were V. and W., the writers, and R., a young woman with a semester's previous training in introspection.

The numerical results appear in the accompanying tables. The first horizontal row, under each observer, marked 'Uncon.', gives the average figures for the experiments where there was no effort to control the rivalry in the original image; the second horizontal row in Table I gives the average results for the experiments where the observer attempted to prolong the blue phase, and the third row those where the attempt was to prolong the green phase. In Table II the second and third horizontal rows give the results of effort to control for blue and red respectively. 'L' stands for the average duration in seconds of each appearance of the color under which it is placed; 'N' for the average number of such appearances. 'M. V.' of course refers to the mean variations of these averages.

The conclusions to be drawn from these tables may be briefly stated as follows:

(1.) In the rivalry of the original stimuli an effort to see either one of the two colors resulted in a lengthening of the average duration of the appearances of that color. The average length of the phase of a given color was longer when the effort was made to hold that color, in every case but two. In one of these exceptions, observer W., when the effort was made to hold the red phase against the blue, obtained an average red phase (2.2 seconds) which was longer than was the case when the series was controlled for blue, but shorter than the red phase in the experiments without attempt to control (4.7). The other exception occurred with observer R., who in the experiments with red and blue got a shorter average phase when she attempted to hold red than under any other circumstances. In the other ten sets of experiments where there was attempt to control, the attempt was successful so far as a lengthening of the average duration of the separate appearances of the colors was concerned. This result is in accord with those of Breese.

(2.) Another of Breese's results is also confirmed. He says, "The rate of fluctuation in the rivalry of after-images is much slower than in the case of direct stimulation." We also find that the average

length of a color phase is longer in the after-images than in the original images.

(3.) A third confirmation of Breese's results relates to his statement that by effort to hold one color in the original image, "the number of fluctuations in the rivalry could not be controlled." Inspection of our tables shows that in every case but two, the number of appearances of a given color was not increased by the effort to see that color, although the duration of the single appearances was. It is a curious coincidence, at least, that the two exceptions are identical with the exceptions to the increase of average phase with effort. Observer W., who did not get a maximum average red phase with the effort to hold red against blue, got the maximum number of appearances of the red phase under these conditions; and observer R., who got the minimum average length of the red phase in trying to hold red against blue, also found that the effort in this direction produced the greatest number of appearances of red. It looks almost as though the effort to control, which normally lengthens the time of each appearance without affecting the number of appearances, might on failing to produce the first effect expend its energy in bringing about the second.

(4.) It will be seen on comparing the figures that when through the effort to hold one color, the average duration of that color in the original image was lengthened, the negative after-image of the color in question was not seen any longer than when no effort at control was made. The lengthening of a color phase in the original image by effort to hold that color is accompanied, in general, by no correspond-

TABLE I

Colors: Green and Blue

Observer W

	STIMULUS								AFTER-IMAGE							
	GREEN.				BLUE.				RED				YELLOW			
	L	m v	N	m v	L	m v	N	m v	L	m v	N	m v	L	m v	N	m v
Uncon.	1.3	.45	9.	1.5	1.7	.22	9.5	1.	3.2	1.3	2.	.5	5.6	2.1	1.2	.3
Blue	1.2	.22	9.5	1.	2.2	.15	9.5	1.5	3.7	.8	1.5	.75	7.4	2.3	2.2	1.6
Green	1.5	.4	9.5	2.5	2.1	.55	9.2	1.8	3.3	.7	1.5	.5	9.5	3.4	2.7	1.7

Observer V

Uncon.	2.2	.27	8.5	1.8	2.3	.8	8.7	1.5	3.3	1.3	3.2	.65	3.3	.9	3.8	1.1
Blue	1.1	.26	7.	1.6	4.4	1.2	7.4	1.4	2.4	1.5	2.7	1.1	3.7	1.3	2.7	.8
Green	3.8	.9	7.8	1.7	1.3	.58	7.2	1.2	4.2	1.2	1.9	.36	2.6	.7	2.1	.5

Observer R

Uncon.	1.6	.47	11.	1.9	2.5	.87	9.9	1.9	1.8	.65	2.8	1.5	3.4	1.4	6.6	1.4
Blue	1.4	.34	9.7	1.5	2.7	.6	9.4	1.3	2.5	.97	2.2	.84	3.2	.78	5.9	1.5
Green	2.2	1.	10.1	1.3	1.8	.29	9.4	1.6	2.5	.98	2.4	.56	3.4	.56	6.	1.4

TABLE II

Colors: Red and Blue

	Stimulus								After-Image							
	Red				Blue				Green				Yellow			
	L	m v	N	m v	L	m v	N	m v	L	m v	N	m v	L	m v	N	m v
Observer W																
Uncon.	4.7	1.8	5.4	.7	2.2	.9	5.	.8	6.3	1.	2.6	.48	4.9	1.5	1.8	.38
Blue	1.8	.32	7.	.8	2.7	.5	6.8	1.	4.6	.7	2.4	.48	4.7	.8	1.8	1.1
Red	2.2	.42	7.8	1.	1.6	.28	7.6	.88	5.1	1.2	2.6	.48	6.	2.5	2.4	.48
Observer V																
Uncon.	1.8	.58	8.6	1.2	1.8	.24	8.6	1.2	6.1	1.9	6.1	1.1	2.4	.44	3.8	.64
Blue	1.2	.22	6.	.8	4.	.48	7.	1.5	4.2	1.	6.	1.	2.8	.78	3.2	.64
Red	3.	.76	7.	1.6	2.	.68	7.	2.	4.5	1.1	5.2	.8	2.8	.8	1.6	.48
Observer R																
Uncon.	2.9	.84	9.	.8	2.1	.78	8.2	.64	3.5	1.2	5.8	1.5	3.4	.98	6.	2.
Blue	2.9	.62	7.4	1.5	2.3	.54	8.2	1.2	3.2	.26	5.2	2.1	2.6	.32	5.1	1.6
Red	2.5	.53	9.6	1.	1.5	.6	9.4	1.1	3.1	.37	5.8	1.7	3.9	.63	4.4	1.6

ing lengthening of the negative after-image of that color. Only two out of the twelve sets of experiments show any trace of such an effect on the after-image. These are the experiments of observer V., with blue and green, which do give a lengthening of the after-image phase corresponding to the control of the original image. As observer V. was the most successful of the three observers in controlling rivalry, it may be that very complete control of the rivalry in the original image might carry with it an effect on the after-image. V.'s control in the red and blue experiments, however, was just as good, and here there was no influence on the after-image.

On the whole, these results are such as we should expect if motor processes condition rivalry. Evidently, when an observer is directed to make an effort to hold a certain color, there is an amount of muscular strain, in addition to the eye-movements which Breese observed under such circumstances, sufficient to make the motor conditions quite different from what they are when the rivalry is merely observed in a passive attitude. As this latter was the attitude during the course of the after-image, it is only natural that there should be little or no correspondence between the phases of original image and after-image.

Another question which demands an answer is this: What is the duration of the average phases in the *uncontrolled* original image, as compared with that of the average phases of their corresponding after-images? The tables show that in the averages there was a certain amount of concomitant variation. A survey of the first horizontal line of figures under each observer indicates that when in uncontrolled rivalry the red phase, for instance, was on the average longer than

the blue phase, the average green phase in the after-image was longer than the yellow one. While this is true of the averages, the detailed records show that it was not true in a majority of the individual experiments, which rather oftener than not fail to point to any correspondence. Perhaps the nearer approach to correspondence between original image and after-image shown in the uncontrolled experiments may be due to the fact that the motor conditions were here more nearly alike in the two halves of an experiment, where no special effort was made in either half; while the fact that a perfect correspondence is very far from existing even in the uncontrolled series may be accounted for by the difference in motor conditions, such as convergence, for example, that must still exist between the part of an experiment where the observer with open eyes looks through a stereoscope, and the part where he watches the after-image changes with closed eyes.

PSYCHOLOGICAL LITERATURE

A Treatise on Facts, or the Weight and Value of Evidence, by C. C. MOORE. Northport, Long Island, N. Y., Edward Thompson Company, 1908. Two vols., pp. clxviii, 730 and 731-1612.

In *Law Notes* for October, 1907, there appeared a rather personal little article by Mr. Charles C. Moore, the author of the work under review, in which the vials of contempt were poured upon Professor Münsterberg's "Nothing but the Truth," a paper which came out in *McClure's Magazine* for September of the same year. I quote a few characteristic passages. "The judge need not exercise, nor even possess, any reasoning faculty. All he needs is faith in the expert. But there 's the rub. Infidelity is rife on the bench." "On almost every topic that has a proximate and practical relation to the trustworthiness of testimony delivered in court, the judges have the psychologists 'beaten a mile.'" "We never find a judge citing or quoting a psychologist." "A treatise on Attention and Memory that would be more voluminous than any entire work on Psychology ever written, could be compiled from the opinions of judges." "Suppose a witness declares he did *not* observe an occurrence. By what reliable means would the psychological expert test that witness in support of a theory that he did observe but has forgotten?" And so on.

The 'psychological expert' was not, of course, seriously affected by this criticism. He knew, first of all, how difficult and how unwelcome is the task of popularizing psychology. There is an old and crusted psychology of common sense which everybody carries about with him; so that the psychologist can hardly open a newspaper, or glance through a magazine, or even sit through a dinner, without receiving dogmatic instruction in some field of his specialty. He could admire, then,—this psychological expert,—Professor Münsterberg's courage in attempting to bring into general notice a growing and practically unknown branch of psychological research; and he could make due allowance for the exaggeration into which the attempt at clearness had led the author of the *McClure* articles. Moreover, he knew that the serious work in the *Psychologie der Aussage* had been published in other languages than English; he knew that this work was being taken seriously by the legal profession in other lands; and he knew that the outside reader would look in vain for any account of it in the pages of Mr. Spencer, President Porter, Dr. Ladd, Ribot, or Dr. James, if only for the reason that an author, however well-reputed, cannot give an account of what does not yet exist. And further, he knew, being a psychologist, that every man is tempted to magnify his own profession; he could make allowance for the legal bias of the student of law; he could take Mr. Moore's *sesquipedalia verba* with a grain of tolerant salt. Finally he knew, what perhaps was more important than anything else, the true character of the original *Aussage*-work, its laborious, cautious experimentation and its hesitant, qualified conclusions. He could be content to let the legal critics furiously rage together, in the assured confidence that his methods would ultimately prove themselves and, so far as they should be proved valid, would ultimately win acceptance.

In the meantime, while experimental work upon the psychology of

evidence is still going on, Mr. Moore has published his two volumes on Facts, or the Weight and Value of Evidence, legally regarded. Here, then, is the lawyer's substitute for a psychology; or rather, here is his applied psychology, compiled in the main from court decisions, but leavened to some slight extent by quotation from non-legal (even psychological) authorities. To accomplish his task, the author "has scanned page by page some thousands of volumes of reports in the United States, Canada, and England." After a general introduction, he presents his materials under the headings: degree of proof; uncontradicted testimony; incredibilities and improbabilities; sound and hearing; light and sight; taste, smell and touch; distance; speed; the weather; course and bearing of vessels; presumptions, inferences and circumstantial evidence; handwriting and typewriting; observation; memory; age, sex, intelligence or insanity of witnesses; affidavits, sworn pleadings and depositions; credibility of witnesses in general; perjury and mistake; bias of witnesses: impeachment, contradiction and corroboration; declarations, admissions, confessions, oral contracts; positive and negative testimony; demonstrative evidence, experiments and photographs; identification of persons and things; expert and opinion evidence; construction of testimony and sundry topics; and weight of evidence in appellate courts. All this material, occupying (with the footnote references) 1,431 pages, lies before me for review. I have read nine-tenths of it, but I cannot do more than offer a few general remarks.

It is clear that no one but an experienced lawyer could tell, without turning up a multitude of references, whether Mr. Moore's compilation has the supreme virtue of works of its class, the virtue of accuracy. I can make no pretence to a judgment on this point. The volumes give every impression of scrupulous care; and I shall assume that this impression is correct. That being granted, the first thing to say is that Moore on Facts is a legal text-book of an entirely novel kind, and one that should prove of immense value to the lawyer in the practice of his profession. The second thing to say is that the making of the book must have involved a truly appalling amount of labor. "The fact that the topics discussed in these volumes have seldom been treated by text-writers, and even more rarely with any citation of judicial authority, is undoubtedly attributable partly to ignorance of the treasures buried in the reports, and partly to lack of courage and industry to pursue a laborious quest for them." The list of cases quoted occupies 116 pp. of double-columned fine print; the list of legal authorities referred to totals, on a rough count, nearer three than two hundred. And the third thing to say is this: that the layman in matters of law cannot but marvel at the range of cast and the fineness of mesh of the legal net, at the scope of the topics discussed and at the minuteness of detail with which these topics are treated. On these three points Mr. Moore's books are deserving of unstinted praise. Their author has also read widely in non-legal literature, the psychological included; he quotes from writers so recent as Thorndike and Stratton and Baird.

Here, however, is the point at which criticism must set in. Mr. Moore has read widely, but he has not read critically, in non-legal literature. The objection is not serious, since one cannot expect a critical judgment from the non-expert; it is, however, regrettable that advice was not sought from—Professor Münsterberg, or some similarly competent psychologist. Modern psychology is so much a matter of the technical magazines, and so little a thing of text-books, that the uninformed reader is exceedingly likely to go astray. Nevertheless, this objection, as I said, cannot be regarded as serious. Much more

important is the fact that Mr. Moore has confined himself exclusively to works and translations in English, and makes no reference whatever to the other modern languages. From the point of view of psychology, and more particularly of the psychology of evidence, this defect is most grave. It is, moreover, a defect that might easily have been overcome by the help of the psychologist *vom Fach*. The literature of the *Aussagepsychologie*—what shall be the English equivalent? the psychology of record, or of report, or of evidence?—this literature is scattered, but it is not bulky.

For the rest, I can, as I have said, only set down a few critical impressions. It is no reflection upon Mr. Moore or upon the law to say that the contents of these volumes are empirical. That, in the nature of the case, they must be. But there can be no doubt that Professor Münsterberg was right in his contention, however severely handled in *Law Notes*, that the applied psychology of the court of justice is "the primitive psychology of common sense." And this it need not be. Mr. Moore's reader is inevitably reminded of the recent controversy between Wundt and Delbrück. The great philologist had remarked, in effect, that he saw a good deal to praise in Wundt's psychology, but that, for his own part, he got along very well with Herbart. And Wundt replied that Herbart was psychologically dead; and that, if the philologists wanted to apply psychology at all, they had better go to something that was alive. Now there is, evidently, a vast deal of applied psychology in the law. Why not, then, apply a modern psychology, a living psychology? Why try to apply that common-sense psychology which it is the first effort of the teacher of psychology to break up in his hearers? Modern psychology has learned, for instance, to draw a clear-cut distinction between experience and report of experience, between the adequacy or reliability of perception, memory, etc., and the adequacy and reliability of the verbal report upon things perceived and things remembered. It has learned, also, to group its materials; it would not have counselled the putting of observation in ch. xvi and the bias of witnesses in ch. xx; or, indeed, the co-ordination of the general method of observation (for observation is simply method) with the specific function of memory. This sort of criticism might be multiplied a hundredfold; but it will do little good until the legal profession shall have been convinced, by aid rendered in concrete instances, that it is at all worth while to transcend the psychology of common sense and to master the rudiments of mental science.

Another general impression that I have gained from the reading of Mr. Moore's volumes is that the position of the expert, in legal regard, is extremely unsatisfactory. First and foremost, I think, because the law makes small distinction between one expert and another, between the empirical familiarity of the handwriting expert and the knowledge of principles possessed by the man of science. But secondly because the expert is often tied down to what I must call illegitimate questions, questions that he would never put to himself and that he can answer, if at all, only conditionally. Every scientific man knows that the outsider's questions are those most difficult to answer; they are for the most part questions of casuistry, and they are more likely than not to presuppose an attitude or a set of postulates which the scientific man cannot adopt or recognize. I feel that I am, in this matter, upon unfamiliar ground, and I hope that I am doing no injustice to legal procedure. My impression is as I have given it; and I cannot help thinking that a body of trained middlemen, versed both in psychology and in the various forms of its application that the law demands, would be as useful here as in education or in psychiatry.

Something, at any rate, is wrong, when Mr. Moore can head a section: 'Expert Opinion Evidence Not a High Grade of Evidence,' and can state that "'expert evidence, while useful in many cases, is dangerous in all,' is a judicial rubric found in substance in many reported opinions." The retort is obvious that this is, itself, an expert opinion, and therefore open to mistrust. But that apart, it is clear that the existence of a body of trained intermediaries between the different sciences and the law would go far to remove "the little confidence [reposed] in the opinions of experts and professors, who often have more knowledge than judgment."

I pass to another point. Modern psychology teaches that the mind is a stream of fluid and changing processes, but that these fluid processes are without exception governed by law, that they evince thorough-going uniformities. A large number of laws or uniformities have already been formulated; and where formulation is still, from our ignorance, impossible, we nevertheless believe that uniformity of occurrence is present. The mind which I seem to see pictured in Mr. Moore's pages is, on the contrary, a static and discrete mind; a mosaic of informations and prejudices; a mind whose furniture, while subject to decay with time, is in meaning and intention permanent, as it were bought to last; and a mind, finally, which is governed by rules that have exceptions. In saying this I am, of course, merely saying in a slightly different form what has already been said: that the psychology of the courts is that of common sense and not of science. The common-sense view of mind comes out, however, with such fatal clearness in Mr. Moore's book that it seems worth while to give this aspect of legal theory a separate mention.

Let me take, by way of illustration, the chapter on memory, which fills 260 pp. and contains more than two thousand citations of cases. The opening section is entitled "The Laws which Regulate the Human Memory." Yet the only law of which one finds mention is the law that memory is obscured by lapse of time. And the passage which the author regards as the most important in this chapter, "or perhaps in the entire work," runs as follows: "Judges prefer to consider all the attending circumstances, and then decide for themselves whether the witness was likely to have remembered, or on the other hand to have overlooked or forgotten." Comment is needless; and it would appear that the author's numerous quotations from psychological text-books are superfluous. As a matter of fact, however, Mr. Moore's opening section does him injustice, for there are many indications of the laws of memory—its dependence on interest and attention, on physical and mental constitution, on age and sex, on the nature and time-relations of stimulus,—in the course of the discussion. As an example of the rule with exceptions we may take the treatment of memory of unpleasant experience. There is a tendency, the author declares, for pleasing reminiscences to take precedence of those that are offensive. *But* there can be no safe conclusion as to the impermanency of disagreeable or painful impressions. I give this illustration, not because psychology has established the laws of memory in respect of pleasantness and unpleasantness, but precisely for the opposite reason, that it has not, and for the collateral reason that it very well can. Here is just the place where the expert, the psychologist of application, might step in, to discover the uniformities that underlie the rule-with-exception of the courts. The unsatisfactoriness of that rule can hardly escape the notice even of the most empirically inclined.

In the above reflections I have been criticising the present status of legal theory and practice rather than the contents of Mr. Moore's

work. My excuse must be that he himself challenged such criticism in his article in *Law Notes*. He invited a comparison between the psychology of memory and attention, as set forth by the psychologist, and its psychology as set forth in the law reports. There is, in fact, no comparison. The psychology of the reports is scrappy and out-of-date. This verdict is less harsh than it sounds, since the same thing might be said of a good deal of recent educational psychology, medical psychology, linguistic psychology, social psychology. All these psychologies, however, are emerging, more or less quickly, from the shadow of tradition and common sense; and with the appearance of the *Aussagepsychologie* the practical psychology of testimony is on the path of progress. Meanwhile, let no jurist suppose that the psychologist underestimates either the magnitude of the task before him or the high level of empirical achievement to which the legal profession has attained. Mr. Moore's work amply testifies, both to his own training and industry, and to the mental caliber of the judges whose views he has assembled. P. E. WINTER.

Buddhism and Immortality. The Ingersoll Lecture, 1908. By WILLIAM STURGIS BIGELOW. Boston and New York, Houghton Mifflin Co., 1908. pp. 75. Price c. 75.

The lecture outlines the teaching of the Buddhism of Northern India and Japan, which is closely allied to Brahminism, as regards human immortality. Briefly put, that teaching is that the self is a resultant of mortal and immortal factors, the former being sensations and emotions, and the latter the will. There is constant conflict between the two opposed forces, and we may assist our own progress towards immortality either indirectly, by the performance of good actions, or directly, by the turning of will upon character. After life in this world we pass to a sort of angelic state, where existence is still personal; and beyond that we reach the ultimate, impersonal peace of nirvana.

The point of greatest interest to the psychologist is, perhaps, the writer's account of the transmigration or re-birth of souls. Sensations are perishable, but sensations often repeated lead to habitual, automatic and reflex action; they thus gain in length of life. Western science then speaks of heredity or atavism, the persistence of a parental or ancestral type; Eastern thought finds illustration of re-birth or re-incarnation. In comparing the two views, Mr Bigelow writes as follows.

"First. If material constitution, that is, inheritance . . modified by the tendency to variation, is the cause of character, then, as the laws of matter do not vary, we have no way of accounting for the tendency to variation itself . . . . whereas, if the psychical characteristics . . . are the dominant factor, the tendency . . . follows as a matter of course.

"Second. If material constitution is the cause of character, the range of variation ought to be equally great in different forms of animal life. . . Whereas [if the soul is dominant], we ought to find the greatest variation where the characters are most complex,—which we do.

"Third. Family resemblance often asserts itself most clearly in the second generation. [And it appears most clearly when the grandparent has been dead less than ten years. Moreover, it appears once only, however many the grandchildren.] Heredity by transmission offers no explanation of either fact. Whereas, from three to ten years is the ordinary interval for re-incarnation, and the single resemblance is the natural result of the re-birth of a single soul." Mendel's Law?

"But it is, perhaps, too early to be sure just what is behind Mendel's law." M. W. WISEMAN.

Die Prinzipien und Methoden der Intelligenzprüfung. Von TH. ZIEHEN. Berlin, S. Karger, 1908. pp. 61.

A reprint, with notes, of a lecture read before the International Congress of Psychiatry at Amsterdam, 1907. The lecture gives an interesting account of the methods used in the testing of defective intelligence in Professor Ziehen's Berlin clinic, accompanied by psychological commentary. First in order stand the tests of retention or deposition: here the author discusses the value for the test of school knowledge *versus* everyday knowledge, the difference between retention of single ideas and retention of ideational complexes, the status of retention in the normal uneducated individual as referred to his everyday knowledge, the relation of retention to *Merkfähigkeit*, etc. A characteristic test is the following: First, a problem in the simple multiplication of one-place figures is given; then six one-place numbers are read off, and the patient repeats them; then six other numbers are read off, and repeated as before; and, finally, the patient is asked to recall the problem set him at the beginning of the test. A typical visual test, in which a geometrical figure is shown for 15 sec. and drawn from memory after an interval of 15 sec., is also described. Secondly, the author speaks of tests of ideational development and differentiation. He instances typical questions for the testing of power of generalization and specification, typical stories told for the testing of the patient's capacity for abstract ideas, questions whose answers presuppose analysis or synthesis or discrimination, etc. Questions that call for a definition he regards as, in general, unsuitable. Thirdly, we have tests of reproduction, carried out by the ordinary methods of the association experiment. Fourthly and lastly come tests of combination. These have a wide range: tests of the patient's orientation in his novel surroundings, tests of inversion of association (months said backwards, *e g.*), tests with puzzle blocks, simple arithmetical tests (easy equations, rule of three), Ebbinghaus' mutilated texts, reproduction of the main feature or main causal relation embodied in a story or picture.

As important conclusions we may single out the following. The question of time, of the rate of intellectual achievement, plays but an inconsiderable part in clinical investigation of the kind here described. There is no certain method of eliminating the influence of grave derangements of association and of emotion. Every test of intelligence should be preceded by a test of attention (cancellation of letters, tachistoscope, mean variation).

Psychologically, the lecture appears somewhat too formal and too clean-cut in its distinctions. The psychology of intellect is still in a very backward condition. To the practical psychiatrist, on the other hand, Professor Ziehen's descriptions and comments will be exceedingly useful. It seems possible, too, that the careful sifting out of tests of defectives may leave a remainder, of valid forms of test, which will be of service to normal psychology as indicating the principal easily differentiable aspects of intellectual function, and thus furnishing rubrics for the study of the normal subject. L. TURLEY.

La pathologie de l'attention. Par N. VASCHIDE et R. MEUNIER. Bibliothèque de psychologie expérimentale et de métapsychie. Paris, Librarie Bloud et Cie, 1908. pp. 117.

The authors of this little essay begin by asserting that we have as yet no normal psychology of attention, and that it will be useful to bring together the experimental results derived from the study of the

attention in abnormal cases. They point out that the only writer upon the psychology of attention who has devoted thought and space to the question of the abnormal is Ribot, and they accordingly summarize the treatment of the subject found in the *Psychologie de l'attention* of that author. The body of the essay is then taken up with the exposition, along with more or less of running commentary, of the methods (reaction time, perimetrical measurement, mental work, fluctuation, æsthesiometry, etc.) employed by various investigators of abnormal attention from de Sanctis to Consoni, Marie, Wiersma, etc. Special chapters are assigned to the work of A Rémond at Nancy (1888) and of Raymond and Janet at the Salpétrière. The names of experimenters, state of their subjects, and reported state of attention, are brought together in a useful chronological table on pp. 108-110. The authors phrase their conclusions as follows. (1) All states of intellectual inferiority, congenital or acquired, stationary or progressive, are accompanied by hypoprosexia. (2) Certain neuropathic states may be accompanied, at least momentarily and exceptionally, by a hyperprosexia, which is itself a condition and not a disease. (3) All mental disorders that take the form of delirium are characterized by paraprosexia. With the experimental means now at our disposal, we cannot give quantitative expression to these qualitatively different morbid or abnormal states of attention. (4) Distraction is either merely a transient hypoprosexia, or an incapacity of attention to maintain itself under some determinate form, *i. e.*, a mental disorientation.

It is clear that these conclusions are very general, and that they give us little insight into the mechanism of disordered attention. Of attention itself we are told only that it stands to the intelligence as reflex irritability stands to the nervous system, and that it presents many different aspects, according as it is spontaneous or voluntary, conscious or automatic, emotive or intellectual, and in these cases conscious or subconscious, etc. A deeper going analysis may be expected from the forthcoming *Psychologie de l'attention* by M. Rivière.

W. JENKINS.

Notes on the Development of a Child. II. *The Development of the Senses in the First Three Years of Childhood*, by MILICENT WASHBURN SHINN. The University Press, Berkeley, Cal., 1907. 258 p.

In this, the second volume of her Notes on the Development of a Child, Dr. Shinn has made a most important contribution to the psychology of infancy. While the source of the original data for the work, as in the case of the author's earlier publications, is the carefully kept record of her own observations upon her niece, this has been supplemented by not only the few scientific records which are available in printed form but by a number of manuscript records obtained through the agency of the Association of Collegiate Alumnæ, of whose child-study committee Dr. Shinn has for many years been chairman. The result of this carefully collated material is the most systematic and complete record of the development of the senses that has yet been contributed to psychology. In the introduction there is a brief discussion of the methods of child study, the principles of classification which have been used by different students of child psychology, including the author's own, of which the guiding idea is that of "*a progressive movement consisting of the integration of simpler activities into more complex and the differentiation of specialized ones out of generalized*, and an anticipatory summary of conclusions.

Under the principle above mentioned the book is divided into four parts. Part I deals with the sensibility of the new born, bringing together and tabulating for comparison the tests and observations of

the best workers in this field on visual, auditory, dermal, taste, smell, muscular, organic and general sensations. The following conclusions are reached.

The condition of the senses in normal infants at birth is as follows:

"1. *Sight* is only a dim and passive feeling of light and dark, without sense of distance or direction.

2. "*Hearing* appears with more or less delay and then only as a dull sensibility to auditory jar, rather than to sound.

"3. *Dermal feeling* includes a sensibility to *contact*, lively about the face (especially the lips and eyes) and duller over the rest of the body; a lively sensibility to decidedly *cold* touches though scarcely to diffused cold; and an exceedingly dull sensibility to pain."

"4. *Taste* and *smell* appear under normal conditions to be almost wholly wanting but can be excited by intensive artificial stimuli. There are also indications that taste, smell and touch are not perfectly differentiated.

5. "Sensations of *motion*, *muscular activity*, *fatigue* and *equilibrium*, *hunger*, *thirst* and *organic pain*, are distinctly felt, but are probably individually feeble. There may be also a faint undercurrent of *general sensation*.

"6. The content of consciousness in the new born is chiefly made up of the sensation of light and of touch and muscular sensations about the mouth and eyes."

These early experiences unassociated with each other or with reproductions of their earlier recurrence, represent the simplest form of consciousness and, since each has its own specific quality, furnish the material for the development of a complex psychic life. At this stage Dr. Shinn finds nothing that "parallels any phylogenetic stage of sense development though traces of the phylogenetic order are evident in the imperfect differentiation of smell, taste and touch, in the low sensibility, to dermal pain and the delayed appearance of cochlear hearing.

Part II discusses the synthesis of sense experience, through which the isolated sensations become fused, grouped and organized into a unified "stream of consciousness." The main lines of this fusion and grouping are as follows:

1. In eye movements, associations are formed between the muscular and visual sensations in the automatic movements of eyes, by means of which the power of voluntarily directing and accommodating the eyes to distance is attained and this is followed by the ability to trace outlines with the eyes leading to the perception of plane form, and rudimentary perceptions of distance, direction and objects as such.

2. During this same period a series of associations is formed between tactile and muscular sensations, especially those which occur in the automatic graspings of the mouth and hand, through which the power of active touch is attained.

3. By a complex process of association, the whole visual-motor group of sensations becomes linked with the tactile motor group experienced in grasping, and through this, aided by locomotion, the perception of solid form and the space-feeling itself are slowly acquired.

4. Auditory sensations become associated with visual and muscular sensations, and associations are also formed between sounds heard and the vocal organs producing them, thus laying the foundation for the development of speech. Admirable summaries and tables of the stages of learning to see, of the development of the active use of the touch organs, and their synthesis are given at the conclusion of these chapters. The development of the auditory sensations, the associ-

tion of sound with sight, and with sound-producing movements are likewise discussed in detail and short chapters are devoted to the associations of the minor special senses and to the development of the feeling of the bodily self, through the growing consciousness of muscular control, equilibrium feelings, exploration of the body surface and organic and general sensations.

Part III, deals with development in descrimination and interpretation and here comes the chapter on the development of color vision in infants, the largest contribution which has yet been made to this almost *terra incognita* of psychologists. As a result of her own observations and their agreement with the reports of other observers, Dr. Shinn concludes that, for at least three weeks after birth, a baby does not see color. Even after sensibility to light had become marked she found no reaction to even the most brilliant field of color. The questions next considered are: (1.) How long does this condition of color blindness last? (2.) By what steps does the child emerge from it? (3.) To what extent dces color perception become developed during the first three years of life? To these questions correspond roughly three stages of the development of color vision in the child. The first is that in which there is no evidence of discrimination and the question to be tested is whether the child really *sees* color or not. Here brightness and contrast must be sharply discriminated from color in order that reactions to color surfaces may not be mistaken for reactions to color, when, in fact they are merely reactions to light. This period extends over the first year and a quarter. In the second period, to be looked for somewhere about the middle of the second year, signs of definite color concepts should appear, if they exist, though this is, of course, quite apart from any ability to understand or use the color names. In the third period, beginning in the latter part of the second year or certainly early in the third, the problem is to ascertain how far the color vision of the child is identical with that of the adult or how far it may still be limited. But few series of experiments have been made on the color vision of young children, those of Prof. Baldwin, covering the ages from 9 to 15 mos. being the first systematic ones. With these the author has compared her own careful experiments and others to which she has had access in manuscript form. The conclusions which she has reached through these data are best given in her own summary.

"The subject is far from being cleared up; but we are at least justified in the following conclusions:

First. The child is insensitive to color at birth and may continue so for several months, though here we have only negative evidence.

Second. Feeble color sensations, beginning at the lower end of the spectrum, and developing progressively upward begin to be felt, certainly within the second half year, and perhaps earlier. These include all the long-wave color sensations by the end of the first year, and probably short wave ones follow soon after, but there is no actual proof of the existence of these before the 18th month.

Third. By the third year, it may be by the latter part of the second, the child has all the color perceptions of the adult, and can be taught to name and discriminate them quite perfectly, and to notice color in the world about him.

Fourth. Colors are seen by the infant more feebly than by the adult as if in a lower illumination. This difference grows progressively less and has nearly disappeared by the third year. It is probably due in part, at least, to the restricted area of the color-sensitive tract of the retina.

Fifth. Pleasure in light precedes pleasure in color; next pleasure

in color appears, depending jointly on their light-richness and their "warmth;" but by the third year the warm colors in some cases lose their advantage, and the cold ones may give as much pleasure. But for color *harmony* no feeling is to be found in the little child."

In this chapter the various space concepts of form, distance, size, direction, locality and solidity, and the child's understanding of pictures are also discussed.

Discrimination in hearing, musical sensibility, sense of rhythm, spontaneous musical expression, and other allied topics, each receive due consideration in the chapter on hearing; touch and the minor dermal senses, taste, smell, muscular, organic, and general sensations are all included among the topics discussed in Part III and the recapitulation of this section, which does not lend itself well to tabulation, is given in 6 month periods, making it exceedingly convenient for reference.

Part IV gives the pedagogical results. These have been foreshadowed by the previous chapters and may be very briefly summarized as follows. In the earliest stage of development, that which comes before grasping, the human presence seems to be the one thing of educational value. As the baby's powers develop the line must be carefully drawn between neglecting to provide necessary stimulus and the danger of overstimulation. In general, the largest possibility of free action is the secret of wholsome and happy development. During the first three years it is in only a slight degree that any formal education can be begun and yet in this period certain psychological foundations for the future may be laid, and a few simple but valuable suggestions are given illustrative of the general principles of such informal instruction.

As a whole the book represents an amount of scientific work and thoroughness that place it in the front rank of psychological investigation. THEODATE L. SMITH.

Ueber Lesen und Rezitieren in ihren Beziehungen zum Gedächtniss, von L. WITASEK. Zeitschr. f. Psychol. u. Physiol. d. Sin., 44 Bd., 1907, pp. 161-185, 246-282.

Groups of ten nonsense syllables were presented visually at the rate of one syllable per second to seven adult observers, who read them aloud in trochaic rhythm. In reciting a group from memory the observer was given the first syllable, and he attempted to recite the remainder in the same rhythm and at the same rate used in the presentation. In case of failure to recall a given syllable, ten seconds were allowed, and then the correct syllable was given him, with the repetition of which he continued the recitation. Designating the number of successive readings in the presentation of a group by Roman numerals, and the number of successive recitations by Arabic gives the following twelve series of different combinations of readings and recitations that were used.

VI+0	VI+5	VI+10	VI+15
XI+0	XI+5	XI+10	XI+15
XVI+0	XVI+5	XVI+10	
XXI+0			

An hour after giving a series it was re-learned through recitations alone, carried out in the same manner as before and repeated to a point where the observer recited the group in ten seconds without error. We may call these the *re-learning*, and the former the *learning* recitations. The degree of memory induced at any point was measured by the time taken to recite, and by the number of forgotten syllables or number of times aid was required in a recitation.

In agreement with results of previous investigations the author found that the impression value (effectiveness for memory) of successive readings decreased very rapidly, this holding true in both the learning and the re-learning recitations. The impression value of successive recitations decreased quite in the same manner. The impression value of a recitation in inducing immediate memory as compared with that of reading far exceeded the latter in all cases. It was greater after the hour's interval when the initial degree of impression already present had been obtained by reading and recitation instead of by reading alone. Likewise, the memory permanency, as measured by the first recitation alone after the hour's interval, was greater for the series that included recitation with the reading. The combination of readings and recitations that resulted in the quickest re-learning showed a complex order, from which the following conclusion is drawn: (1) The optimum total time for reading equals about one-fourth of the total time spent on reading and recitation together. (2) The optimum division of the total time spent for recitation between learning recitation and re-learning recitation is one that gives less than half to the former.

The figures on which these generalizations are based are in most cases derived in several different ways, the possibility of which may be seen from the nature of the series that were given.

F. KUHLMANN.

Expériences sur le rôle de la récitation comme facteur de la mémorisation, par M. DIMITRE KATZAROFF. Archives de Psychologie, 1908. pp. 225-258.

Couplets of nonsense syllables, eight to ten in a group, were presented visually at the rate of one couplet per two seconds to six adult observers, who read the syllables aloud in trochaic rhythm. The presentation of a group was combined with recitations in which the first syllable of a couplet was presented visually, while the observer tried to recall the other, and failing to do so was given the term orally. The final recall, in which the first term of a couplet was again presented as in a recitation, followed 24, 48, or 72 hours after the learning. Using Roman numerals to designate the number of consecutive presentations and Arabic figures for the number of recitations, gives the following combinations, with the intervals before recall, that were employed:

Combinations				
A	B	C	D	E
X+I+V	VIII+VII	VIII+VII	IV+VI	IV+VI
X+I+5	VIII+7	VIII+I+VI	IV+6	IV+3+III
			IV+I+I+I+I+I+I	IV+I+I+I+I+I+I+I
Intervals				
48h.	72h.	72h.	24h.	24h.

The degree of memory induced in the different series was measured by the number of terms recalled, and by the time taken to recall one. Thus measured, the author found that the fixation value of a recitation is greater in all cases than that of a presentation. Syllables that were recalled in the first recitation after the first group of presentations (four to ten) were remembered best, in the final recall, when the relative number of subsequent recitations was greatest. This held true also for syllables that were not recalled in the first recitation, but were recalled in the later ones. The number of syllables recalled in the first recitation but forgotten in the final recall, was greater than the number not recalled in the first recitation, but learned later and remembered in the final recall. This is explained by assuming that the first recitation in a

series shows the observer what he has learned and what he has not learned, resulting in a subsequent concentration of the attention on the latter and a neglect of the former. The best combination of presentations and recitations seems to be one that includes at least two recitations, one for showing the observer what he has already learned and another for memorizing proper, this number depending on the amount of material and the number of previous presentations. Then at least two readings should follow to reinforce the perception of the material as a whole. The recitations should come together, instead of alternately with presentations. Further explanation of the superiority of the recitations is found in (1) a difference in the attitude of the observer in the presentations and the recitations; (2) in the unfamiliarity of an isolated term of a couplet when it has not been presented alone before as in a recitation, (3) in the fact that the neural processes are probably the same in the recitation and the recall, while in a presentation they differ from both. F. KUHLMANN.

Ueber Vorstellungstypen, von L. PFEIFFER. Pädagogische Monographien, II, Bd., 1907, pp, 1-127.

The first eighty-five pages of this monograph are given to review and discussion. The methods and results of investigations on ideational types in verbal, and in concrete thinking are comprehensively and critically considered. This includes studies whose aim and methods were not primarily directed to the investigation of ideational types, but from whose results some inferences as to ideational types can be made. The next twenty-eight pages are given to the presentation of results of the author's own experiments, followed by a discussion of the theory of ideational types, and of their practical significance to the teacher.

The experiment consisted of the subjects' writing down the first suggestion (indicating also the nature of the imagery) aroused by a word pronounced to them by the experimenter. Four classes of words were used, a group of ten of one class being presented at one sitting per week. (1) Nouns with a predominant visual and auditory content. (2) Verbs with a predominant visual and auditory content. (3) Nouns with a predominant kinæsthetic content. (4) Verbs with a predominant kinæsthetic content. Two groups of ten words of each class were used. The test was made the first year on a class of fifteen girls with an average age of ten years. It was repeated on the same girls (with several changes in the class) the second and third years, using the same words as material. Classifying the suggestions aroused by these words as visual, auditory, and kinæsthetic gave the following percentages belonging to each:

	Visual	Auditory	Kinæsthetic
1st year	56	31	12
2nd "	47	31	21
3rd "	50	30	20

It is to be remembered that half the words used were designed to suggest kinæsthetic imagery. Taking the middle values for the class for the number of times each kind of imagery was suggested gave

	Visual	Auditory	Kinæsthetic
1st year	44	25	10
2nd "	39	26	17
3rd "	40	23	14

Taking these middle values as a norm gave the following percentages of the number of children whose imagery exceeded this norm in the three classes:

	Visual	Auditory	Kinæsthetic
1st year	47	33	20
2nd "	40	25	35
3rd "	47	18	35

The author concludes that the ideational type is fairly constant, but discusses the small changes that, according to the figures, occurred. The monograph is a valuable one chiefly for its critical survey of methods and previous results. F. KUHLMANN.

The Nervous Correlate of Pleasantness and Unpleasantness, by M. MEYER, Psychological Review, July and September, 1908.

The author considers that the present confusion in the psychology of feeling is due in no small measure to the lack of attempts to determine the nervous correlate of feeling, which for him is the same as the nervous correlate of pleasantness-unpleasantness. To emphasize this confusion he summarizes the views of Lagerborg, Marshall, Stumpf, Fite, Lipps, Alechsieff, Miss Calkins and Pikler.

He then prepares the way by a theory of the structure and function of the nervous system, stated in mechanical terms, the essential point of which is a comparison of the nervous system to a very complex system of pipes filled with fluid, so interconnected through higher centres that an impulse given to the contained fluid at any point can be transmitted through the ramifications of the pipes to any other point. It is assumed that the resistance of a pipe or series of pipes often used will decrease, while that of a series seldom used tends to increase. If, for any reason, two stimuli varying in intensity are given simultaneously, the more intense tends to attract the lesser to its own path, whose resistance is thus decreased until it becomes the path preferred whenever possible, causing thus possibly a motor reaction different from that expected. From this point of view the author explains the phenomena of habit, variation and sensory and motor condensation.

This current within the system of pipes is evidently the nervous correlate of sensation. But feelings of pleasantness-unpleasantness arise only when two simultaneously existing currents meet in the higher centres in such a way that the total activity is increased, (causing pleasantness), or decreased (causing unpleasantness). The more complicated the structure, the more opportunity for pleasantness-unpleasantness to arise; it thus belongs in its definite form to a high stage of evolution.

The author points out that this view explains the fact that certain usually unpleasant sensations may through habit or purpose become pleasant, and *vice versa*. It also explains the lack of images of pleasantness-unpleasantness as well as the fact that these states cannot occur without perception; but conceivably, through complexity of structure, pleasantness and unpleasantness may exist at the same time. Emotions, according to this view, are not entirely derivable from pleasantness-unpleasantness, and may, indeed, exist without them. It is also evident that pleasantness and unpleasantness are not, and cannot become, sensations. H. W. CHASE.

A Theory of Mind, by JOHN LEWIS MARCH. Scribner's Sons, New York, 1908. 453 p.

The writer tells us "that many believe that the next great advance should take place in psychology, and that this advance should be the result of a clarification of the field chiefly by the modern science of biology," which has hitherto had an extraordinarily slight influence in this direction. "Biology and psychology still stand almost rigidly

apart." The fault, however, is with the psychologists, and the author here proposes a theory of instinct intended to meet this situation and show what the fundamental misconception has been and in so doing to break the deadlock and allow the scientists to come together. In the words of the author, the theory "has been accomplished almost in solitude and I have little to say in the way of special acknowledgments." He assumes that both mind and matter are found in unities that are similarly limited in space and in their complex forms in time. The simpler unities of mind are found in connection with the simpler unities of matter, and the complex unities of mind with the complex unities of matter. Mind is not known to exist apart from matter. The phenomenon of the latter can be fully explained with no reference to mind, which knows nothing of matter except through experience. Mind seems affected only through physical means. Instead of saying of matter and mind that either controls the other it is better to say that matter to itself is mind, mind as it reveals itself to another mind is matter, and so they are thus to be considered as identical. The atom has an impulse to fuse with certain other atoms, to influence and be influenced by them. Now an impulse is identical with the setting free of force. The influence emitted in an impulse is such as to bring about a complimentary impulse in a suitable other mind and to make this impulse definite. The act of satisfying an impulse is identical with the movement in space, and the force tends to the motion necessary to bring about the satisfaction of the impulse. When an impulse is satisfied or its results broken up, the equilibrium of forces is accompanied by a feeling of pleasure or pain. In the building up of mind, impulses are specialized out of more generic ones by fusion and subfusions. The results show that memories are not inherited but impulses are. Instincts are thus not the results of experience but are original. Instincts are in a general way similar, and many of our impulses are satisfied indirectly or are not entirly satisfied. We have a cell material and a cell personal instinct, a body material, personal and social instinct, recognition instincts, thought instincts, ideal instincts and many combinations of these. Thus out of the individual, whether cell or personal, higher unities are ever being evolved.

The writer seems to us to be correct in recognizing the biological foundations of psychology and also in his feeling that when, in the good time coming, human instincts shall be treated in the same objective way as those of animals, the chasm between the two will close. But his method seems to us abstract, formal, and far too little illuminated or even informed by facts. To our thinking, the way to demonstrate his fundamental thesis would be to parallel, step by step, the latest situations, for instance, of human society, and those of animal social organizations from the ant up, and so with all the rest; and the success of such an effort will depend entirely upon the mastery of the facts in both these fields, and that this author hardly seems to us to possess.

Principles of Psychic Philosophy, by CHARLES B. NEWCOMB. Lothrop, Lee, & Shepard Co., Boston, 1908. 199 p.

This book is dedicated to those "who are beginning to understand that in this present mortal life man has the opportunity of unfolding all the powers and realizing all the privileges of any spiritual plane." To realize these higher powers of man the author suggests that "two days be given to the careful consideration of each chapter." This would require twenty-four days as there are twelve chapters, viz., God, Nature, Man, Psychism, Suffering, Selfishness, Responsibility, Adjustment, Power, Freedom, Healing and Fulfillment." If we take

the first chapter as an example, we must recognize everlasting arms beneath us; a universal force superior to ourselves governing life, intelligent, benevolent, supreme, silent, irresistible, must discover every point of development. The telescope and microscope are harmonious. Supreme good, love, wisdom, law, are synonyms. Every alternative is between law or chaos, design or accident; and GOD GEOMETRIZES. This will perhaps suffice to show the general point of view of the author and his method. He wishes every one to achieve the highest good, realize the highest knowledge, evolve himself or herself to the uttermost, and the method of accomplishing these magnificent ends is to meditate along the lines of his pregnant sentences until all the truth that they contain is irresistibly borne in upon the soul, which will thereby be greatly advanced toward its goal.

Ueber Theodor Lipps' Versuch einer Theorie des Willens. Eine kritische Untersuchung, zugleich ein Beitrag zu einer dynamischen Psychologie, by JULIUS PIKLER. Barth, Leipzig, 1908. 50 p.

This critical investigation is at the same time a contribution to a dynamic psychology and is directed chiefly against Lipps's "*Vom Fühlen, Wollen und Denken.*" The author expresses the wish that his treatise be read especially by those who have already perused his work on "*Das Beharren und die Gegensätzlichkeit des Erdlebens.*" He then proceeds to treat, 1—Striving and the conviction of possibility, 2—Striving as the actuality of objective tendency to realization. And in a supplement he treats of the relation between ideas and experience.

Der Begriff des Ideals; eine historisch-psychologische Analyse. WILHELM ENGELMANN, Leipzig, 1908. 136 p.

This historico-psychological analysis is a doctor's thesis at Erlangen. The writer first traces the history of the various theories of the ideal beginning with Spinoza and ending with Ribot, Wentscher, Cohen and Ricardo. The author thinks light can be shed upon his general problem by means of two questionnaires which he appends, both of which cover several pages and are too lengthy to be reproduced here. He wishes to know how many people think seriously over sense impression, whether they love society or solitude, are fond of expressing themselves, really suffer from doubt, what kind of a youth they spent. This to the end of getting their personal ideals. The second questionnaire is characterological and pertains to selfishness, pedantry, faithful memory, presence of mind, casuistry, fancy, receptivity, excitability, friendship. To these are appended other queries of a still more detailed character, as to the attitude of the individual toward ideals.

Vortex Philosophy, or the Geometry of Science, by C. S. Wake. Published by the author, Chicago, 1907. 36 p. Diagrammatically illustrated.

This is a geometry of science, diagrammatically illustrated. It has been evolving in the author's mind since 1892 and was suggested by Mr. J. J. Van Nostrand. This is the epitome of a manuscript of 600 pages that was burned, and hence there is much for which the scientific data is not given in this pamphlet. The author expresses some obligation to Ribot, Haeckel and Royce and has a mechanical device illustrating his theory. This paper is illustrated by many rather intricate cuts and diagrams, some of them colored, and everything from the motion of primitive elements up to sexuality is explained.

Questions in General and Educational Psychology, by GUY MONTROSE WHIPPLE. C. W. Bardeen, Syracuse, N. Y., 1908. 197 p. (Cornell Study Bulletins for Teachers, No. 3.)

The range of these questions is wide and interesting. It covers the nature and scope of psychology, mind and body, heredity, acquired forms of response, sensory-motor mechanism, the general principles of mental elaboration, perception and observation, memory, imagination, conception and language, judgment, apperception, the affective process, formal discipline or general training, abnormal and anomalous psychoses. This bulletin is the outgrowth of an attempt on the part of the writer to supply his students with the means for checking up their progress in psychology, and for securing their intelligent assimilation of the material supplied by lectures, reading, and classroom discussions. Believing as we do that this is a very great need in the teaching of this subject at the present time, especially in view of the prevalence of the lecture method, we can but welcome this text, even though it seems to us, from a rather cursory reading, to be somewhat uneven in its merits.

Ethics, by JOHN DEWEY and JAMES H. TUFTS. Henry Holt & Co., New York, 1908. 618 p.

This text strives to awaken a vital conviction of the general reality of moral problems and the value of reflective thought in dealing with them. The historical material in Part I is subordinated to this end. Part II is devoted to different types of theoretical interpretation and Part III to typical social and economic problems which characterize the present. It is hard for the student of morals to get the subject objectively and definitely before him, so that the problems seem real. Conduct is so intimate that it is hard to analyze. Hence all must be concrete, and yet the classical conceptions of moral theory are of remarkable importance in illuminating the obscure places of the moral life and in giving the student clues that will enable him to explore it for himself. The authors do not aim to inculate a ready-made system, and they recognize that in the political and economic portions of Part III no definite treatment is yet possible. It should be added that the first two hundred pages, or Part I, are by Professor Tufts; and the third part (pp. 427-606) by both authors. For each main topic, a list of important literature is given, and there is a well-made index.

Psychotherapy. A course of reading. Combining sound psychology, sound medicine and sound religion. Vol. I, No. 1. Centre Publishing Co., New York, 1908. 100 p.

This admirably printed journal as edited by W. B. Parker and published by the Centre Publishing Company, 30 Church Street, New York, contains articles by Dr. Richard C. Cabot, J. J. Putnam, Rev. L. W. Batten, Prof. J. R. Angel, F. T. Stimpson and Rev. Lyman P. Powell. Others whose co-operation is enlisted are Prof. Jastrow, Bishop Fallows, Prof. Royce, Prof. Woodworth, and Dr. F. Peterson. The opening article is by Dr. Cabot, who speaks of psychotherapy in Europe, strangely enough, without even a mention of the Freud school, but declares that psychotherapy "has its place, not instead of, but by the side of, chemical and physical methods." He advises those interested to associate with the Harvard psychologists and thinks we need team work. Dr. J. J. Putnam holds that man is a psychophysical organism, that the right philosophy is important, that materialism and pessimism are current and should be resisted, refers to the work of Loeb, Sherrington, Bergson, and also has a word to say about pragmatism, will, faith, etc. Another article by Dr. Batten characterizes healing in the Old Testament. Professor Angel treats of the relations

between mind and body, especially the brain, and concludes that mental healing is possible and that its methods deserve recognition, emphasizing suggestion and also speaking of caution in view of dangers. Dr. Stimpson then describes the nervous system beginning with the neurone and then characterizing the brain as a democracy, speaking of evolutionary centres, the powers of those that are high and low, brain architecture, influence of conscious mind on lower centres, the secondary self or dual personality. And finally Dr. Powell describes psychotherapy in Northampton, how he applied the Emmanuel method and some of the results achieved, among which was one hopeless case.

All these articles are broken up into brief sections with captions, are accompanied by notes, collateral reading, editorial summaries, etc., so that it is all brought down to the most elemental mind by all kinds of notes; and a glossary of technical terms, such as catalepsy, cell, cerebellum, cerebral, cerebrum, ganglia, neural, prognosis, phonation, soporific, sensory nerves, etc., follows.

Das Sexualleben des Kindes, von DR. ALBERT MOLL. Walther, Berlin, 1909. 313 p.

Moll ranks with Havelock Ellis, Krafft-Ebing, and Freud perhaps in expert knowledge of this subject. After a historical introduction, the subjects treated are as follows: the organs and the instinct of sex; sexual differences in childhood; symptomatology, pathology, etiology, and diagnostics; the meaning of sexual life for the child; the child as an object of sexual treatment; education in sex. The topics are treated in a very practical and able way.

Das Sexualleben der Australier und Ozeanier, von DR. B. SCHIDLOF. Hallberg, Leipzig, 1908. 314 p.

This is a very painstaking and luminous contribution to what the writer calls "the new science of sexual psychology." The chief topics discussed are: the sexual life of early childhood, the ceremonies of pubertal initiation for boys and for girls, the sense of shame and modesty, prostitution and concubinage, sexual aberrations, diseases, ideas of beauty, erroticism in costume, dancing, song, love charms, virginity, marriage and its forms and violations.

Race or Mongrel, by ALFRED P. SCHULTZ. L. C. Page & Co., Boston, 1908. 396 p.

This is a brief history of the rise and fall of the ancient races of the earth, and a theory that the fall of nations is due to intermarriage with alien stocks. The argument is that national strength is due to racial purity, and the writer concludes that America will sink to early decay unless immigration is rigidly restricted. The twenty-nine chapters take a wide range, from the Hamites, Phœnicians, Egyptians, Jews, Gypsies, Greeks and Lombards to the Anglo-Saxons, the present yellow races, the German-Americans, the pan-Europeans in this country, and the American negro. No historic race was ever destroyed by inbreeding; and no race that practiced it was ever destroyed from any cause. The English to-day are the strongest European race. They crossed with Danes and Normans but the immigration never amounted to an inundation and the immigrants were pure and closely related, while the absorption was slow. Race is everything. A world language is bad because it bastardizes those who speak it, and every man's tongue is no man's tongue. It is a language spoken by mongrels, and every mongrel is worthless. If uniformity, eternal peace and bastardization of all nations is devoutly wished, then let us spread the English language.

Le Spiritisme dans ses Rapports avec la Folie, by MARCEL VIOLLET. Librarie Bloud & Cie., Paris, 1908. 117 p. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

The first chapter considers spirits—normal, predisposed and foolish; in successive chapters, the phenomena and the doctrine of spiritism are discussed; later comes mediumopathy, external and internal, and mediumomaniacs. The insanity of spiritism is classified as 1. deliriums of spiritistic origin; 2. follies not of spiritistic origin but colored with delirium. Then follow certain conclusions.

Les Hallucinations Télépathiques, par N. VASCHIDE. Librarie Bloud & Cie., Paris, 1908. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

This is a very interesting study which the author carried on for nine years on two series of people: the first 21, and the last 13, in number, using in all, in the first case, 1,011, and in the last, 363 determinations. He found many veracious cases, but strongly advocates the view that there are no spiritual agents involved, but that credulity, the social *milieu*, cleverness and experience, and malobservation on the part of the beholder, are sufficient to account for all the facts. The lowest class of people in France are just nine times as prone to believe in supernatural interpretation as the most cultivated class.

Diseases of the Nervous System, by ALFRED GORDON. P. Blakiston's Sons Co., Philadelphia, 1908. 487 p.

This work, with its one hundred and six illustrations, is designed especially for the general practitioner and for the student. The writer believes that neurology has not been made sufficiently attractive, owing to the too technical treatment it has had. This work aims to give a plain, practical account of the diseases of the nervous system. The first chapter is on anatomy and physiology; the second, methods of examination and diagnosis; then follow chapters respectively on cerebral localizations, apoplexy, encephalitis, Jacksonian epilepsy, aphasia, hemianopsia, tumors, hydrocephalus, diseases of the basal ganglia, circulatory troubles, diseases of the cerebellum, medulla, pons, cord, periphery, syphilis, paresis, functional troubles, diseases of the sympathetic system, and intoxication.

Neurological and Mental Diagnosis, a Manual of Methods, by L. PIERCE CLARK and A. ROSS DIEFENDORF. The Macmillan Co., New York, 1908. 188 p.

This volume is designed to aid the student and general practitioner to make thorough and systematic examinations in nervous and mental diseases. A definite method of procedure is laid down in both lines of examination, in order that proper analysis may be made easily and readily in routine case study, either in hospitals or private practice.

Untersuchungen zur Kenntnis der psychomotorischen Bewegungsstörungen bei Geisteskranken, von DR. KARL KLEIST. Leipzig, Klinkhardt, 1908. 171 p.

This is a careful work dedicated to the memory of Wernicke. It is based on a clinical study of cases in the nervine hospital at Halle. It treats of motility, akinesis and psycho-motor, as opposed to cortical, transcortical and ideational apraxia. Motor aphasia is also laid under tribute. Psycho-motor disturbances of innervation are dwelt upon with reference to their low position. And here imperative movements and attitudes are discussed along with asthenia. The basis of Wernicke's body consciousness is the reflex system. These disorders, with their complex symptoms of strain and excitement, modify feeling, attention and thought, all of which are connected with each other and with the cerebellar and frontal brain system.

The Psychology of Advertising, by WALTER DILL SCOTT. Small, Maynard & Co., Boston, 1908. 269 p.

This work is by the author of "The Theory of Advertising," published in 1893, and is dedicated to American business men who wish to be scientific. After treating the memory, feelings and emotions, the writer points out how an advertisement must appeal to the customer's sympathy, must show human instincts, be suggestive, start up the will, favor the habit of reading advertisements following the law of progressive thinking, must be scientific in rightly estimating the attention value of small and of large spaces, etc. He then discusses the mortality rate of advertising, the psychology of food advertisements, the unconscious influence in street railway advertising, questionnaire method illustrated on newspapers; and the work ends with a bibliography of the subject, which occupies some twenty pages. The book abounds in shrewd and rather new suggestions, and is illustrated.

The Story-Tellers' League. The Story Hour. Vol. I, No. 1. November, 1908. Edited by W. C. Ruediger, Ph. D., with Richard T. Wyche as Consulting Editor. Published ten times a year, Washington, D. C. Subscription price, one dollar. Address—The Story Hour, 406 5th St. N. W., Washington, D. C.

It is to our thinking high time that the Story-Tellers' League, numbering some five thousand people in different parts of the country, should have its own organ, and it is well also to begin in this modest way. The articles in this number are not ambitious but are telling. There are reports of the League work in different centres, a number of book reviews, the constitution and by-laws of the League, and one or two sample stories are told.

BOOK NOTES

Beobachtungen über die Psyche der Menschenaffen, von ALEXANDER SOKOLOWSKY. Neuer Frankfurter Verlag, Frankfurt a. M., 1908. 78 p.

This is a careful study of the psychic life of the gorilla, chimpanzee and the orang utan, made at first hand. The writer has had many of these animals under observation and has studied them carefully. The conclusion of his very interesting study is that apes and man differ from each other morphologically and psychologically only in degree and that the divergence is on a unitary basis.

The Hearing of Primitive Peoples, by FRANK G. BRUNER. The Science Press, New York, 1908. 113 p. (Archives of Psychology, No. 11, July, 1908.) (Columbia Contributions to Philosophy and Psychology, Vol. XVII, No. 3.)

This study is divided into upper mediums of hearing and auditory acuity. The historical sketch introduces each part, followed by characterization of instruments. The data are from the Indians, Filipinos, Ainus and Pygmies. In general, the Filipinos lead, then follow the Ainus, with whites lowest of all.

A Study of the Influence of Custom on the Moral Judgment, by FRANK CHAPMAN SHARP. 1908. 144 p. Bulletin of the University of Wisconsin, No. 236.

The writer has here made rather suggestive use of questionnaires upon various affairs in which custom affects our judgment, and finds a good deal of diversity of opinion and a wide, general distinction between honest, personal judgments on the one hand, and custom on the other.

The Problem of Form in Painting and Sculpture, by ADOLF HILDEBRAND. Translated and edited by Max Meyer and Robert M. Ogden. G. E. Stechert & Co., New York, 1907. 144 p.

The author discusses vision and movement; form and appearance; idea of subjects and its visual expression of balance and depth; conception of relief; form as an interpretation of life; and sculpture in stone. The book has few illustrations.

Les Synesthésies, par HENRY LAURES. Librarie Bloud & Cie., Paris, 1908. 97 p. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

The author first treats of colored audition; then of the other synesthesias of the same type; then of those of an emotional character.

L'Audition Morbide, by A. MARIE. Librarie Bloud & Cie., Paris, 1908. 146 p. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

The first class of morbid auditions are those by deficit or hypoacousias, and the next are those by excess or hyperacousias. These are described in an interesting way, and a general bibliography follows.

Les Préjuges sur la Folie, par la PRINCESSE LUBOMIRSKA. Librarie Bloud & Cie., Paris, 1908. 146 p. (Bibliothèque de Psychologie expérimentale et de Métapsychie.)

The prejudices are characterized as follows: 1. that insanity is of supernatural origin; 2. that fools show their folly; 3. that insanity is dangerous; 4. that it is incurable; and 5 that it is contagious.

Concerning Lafcadio Hearn, by GEORGE M. GOULD. Fisher Unwin, London, 1908. 303 p.

This is an interesting and in some respects ideal biography, describing heredity, the gruesome period, getting a soul, the development of the author's poetic talent, with style, appreciation, etc.

Twenty-sixth Annual Report of the Bureau of American Ethnology, 1904-5. Gov't Printing Office, Washington, 1908. 512 p.

This work is chiefly devoted to a study of the Pimas Indians by Frank Russell, with forty-seven plates and over a hundred figures; and to the social conditions, beliefs and linguistic relationship of the Tlinkit Indians, by J. R. Swanston.

Ursprung und Wesen des Menschen, von KARL CAMILLO SCHNEIDER. Franz Deuticke, Leipzig & Wien, 1908. 125 p.

The writer first gives the basis of phylogeny and the story of humanization. The second part discusses spiritual substance, idea, understanding energy, logic, exonœsis and endonœsis, will, feeling, ego, non-ego, idea and work, and finality. The work contains various cuts and tables.

Technical Aspects of Experimental Psychopathology, by FREDERICK LYMAN WELLS. Rep. from American Journal of Insanity, Vol. LXIV, No. 3, Jan., 1908, pp. 477-512.

Chemical Problems in Hospital Practice, by OTTO FOLIN. Rep. from the Journal of the American Medical Association, May 2, 1908. Vol. L, pp. 1391-1394.

On the Variability of Individual Judgments, by FREDERICK LYMAN WELLS. Rep. from Essays Philosophical and Psychological in Honor of William James. New York, 1908. pp. 511-549.

Die Wirkung von Suggestivfragen, von OTTO LIPMANN. Johann A. Barth, Leipzig, 1908. 169 p.

NOTES

THE CONGRESS OF THE JAPANESE SOCIETY FOR CHILD STUDY

The Japanese Society for Child Study, which was established twenty years ago and was recently re-organized, held its congress on May 10 and 11, 1908, at the Imperial University of Tokio with an attendance of about 250 members of various callings, school teachers, physicians, psychologists, ministers, criminologists, lawyers, etc. Dr. Y. Motora, Professor of Psychology in the University and President of the Society, spoke, in his address, on the purpose of the society and of its past services. Papers were presented by the members on the following subjects: K. Sugawara—On the æsthetic feelings of school girls for cherry blossoms. Dr. S. Warashina—On hysteria in infancy. I. Miabe—On backward children in the common schools. S. Sawaki—On psychopathic feeble-mindedness. G. Roseki—On the mental states of school children. Y. Ohara—On vacation colonies. Dr. S. Kurahashi—Children and poetry. A Sennichi—On the heredity of myopia, Kishibe—The crying of children and its treatment. Dr. K. Yoshida—New view-points of child study in Germany. F. Mayeda—Mimetic expressions of the child. Dr. T. Ishiwara—Mental development of the child. Dr. I. Shimoda—Children in the city and in the country. N. Miyamoto—Convulsions in children. Dr. H. Takashima—On the influence of pedagogy upon the mental states of youth. Dr. Y. Fujikawa—On nervousness in infancy. Dr. A. Miyake—On criminal youth as result of disease. Dr. S. Asoh—Present condition of the higher education of women in Europe. Dr. T Yamada—On the question of overtaxation. Dr. T. Fukurai—"Isolated" psychical functions. Dr. Y. Motora—On mental gymnastics. Dr. H. Miyake—Some remarks on child study.